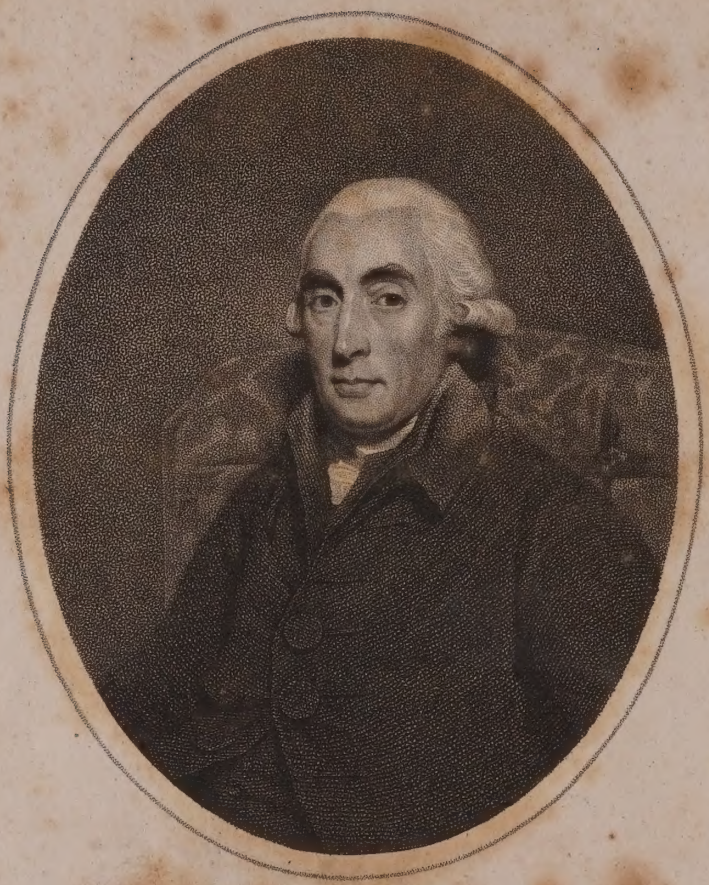
H. 6. 6
6
24

65962/C
The Library of the
Wellcome Institute for
the History of Medicine

MEDICAL SOCIETY
OF
LONDON
DEPOSIT

Accession Number

Press Mark
BLACK, J



Dr. JOSEPH BLACK.

Published by Longman & Rees, May 2, 1803.

MEDICAL SOCIETY OF LONDON

LECTURES

ON THE

ELEMENTS OF CHEMISTRY,

DELIVERED

IN THE UNIVERSITY OF EDINBURGH;

BY THE LATE

JOSEPH BLACK, M.D.

PROFESSOR OF CHEMISTRY IN THAT UNIVERSITY,

PHYSICIAN TO HIS MAJESTY FOR SCOTLAND; MEMBER OF THE ROYAL SOCIETY OF EDINBURGH, OF THE ROYAL ACADEMY OF SCIENCES AT PARIS, AND THE IMPERIAL ACADEMY OF SCIENCES AT ST. PETERSBURGH.

NOW PUBLISHED FROM HIS MANUSCRIPTS,

BY

JOHN ROBISON, LLD.

PROFESSOR OF NATURAL PHILOSOPHY IN THE UNIVERSITY OF EDINBURGH.

VOL. I.

EDINBURGH:

PRINTED BY MUNDELL AND SON,

FOR LONGMAN AND REES LONDON, AND WILLIAM CREECH EDINBURGH.

1803.

Mag. 4-6.

TO JAMES WATT, ESQ.

OF HEATHFIELD, NEAR BIRMINGHAM.

DEAR SIR,

BY placing your name in the front of this edition of the Lectures of our excellent Master, I think that I pay my best respects to his memory, and also do a service to the Public. By thus turning the Reader's attention to Dr. Black's most illustrious Pupil, I remind him of the important services derived from his discoveries: For surely nothing in modern times has made such an addition to the power of man as you have done by your improvements on the steam engine, which you profess to owe to the instructions and information you received from Dr. Black.

When I contemplate the unparalleled state of prosperity of the British empire, resulting from the skill, spirit, and activity of its inhabitants, and reflect on the imperious call now upon us for still greater exertions, that we may maintain ourselves in this our envied preeminence, I feel it my duty to hold forth every incitement that can animate to this honourable emulation. I shew the Reader, in your example, that there is no preeminence in scientific attainment which he may not hope to reach, by rigidly adhering to the sober plan of experimental in-

quiry, ſo conſtantly inculcated by Dr. Black; and turning a deaf ear to all the faſcinating promiſes of ſplendid theories. The ſpark, which I thus throw out, may chance to light among ſuitable materials,—ſome felices animæ, quibus hæc cognoſcere curæ eſt,—*minds perhaps unconſcious of their own powers. Even your's might have lain dormant, had not Dr. Black diſcovered its latent fire.*

I acknowledge that I have alſo another inducement. I wiſh to be known as a perſon who enjoyed a ſhare in the good opinion, and the uninterrupted friendſhip of two of the moſt eminent philoſophers and moſt worthy men of the age ana nation.

That you may long continue to enjoy your well earned honours, happy in the ſociety of your friends, and ſtill increaſing the means of the national proſperity by your inventions, is the earneſt wiſh of

Dear Sir,

Your affectionate Friend,

And obedient humble Servant,

J. ROBISON.

Edinburgh, April 7. 1803.

THE EDITOR'S PREFACE.

THAT I engaged to reviſe and prepare for publication the prelections of this eminent Profeſſor, may appear preſumptuous, and to require ſome apology. Chemiſtry is a ſcience of ſuch immenſe extent, ſo multifarious, ſo abſtruſe in its principles, and intricate in their combination and mutual dependence, that to pretend to appreciate, or, if neceſſary, to alter any thing written by Dr. Black, requires no common ſagacity, and a degree of information not to be looked for in one who is not profeſſedly a chemiſt. And it ſeems a taſk too great for any perſon ſufficiently occupied in official duties of a very different nature. I acknowledge the juſtice of the charge.

But I truſt that when the reader, and particularly thoſe who have had the happineſs of liſtening to the prelections of this excellent teacher, is informed in what manner this taſk fell into my hands, the appearances of unwarrantable preſumption will be conſiderably leſſened, and that my endeavours to perform it in a ſuitable manner will be received with ſome indulgence.

Immediately after the deceaſe of my worthy preceptor, colleague, and friend, his executors notified their intention of publiſhing an edition of his lectures from his manuſcript notes. This was done in order to prevent a publication, which, they were informed, was in-

tended from notes taken by his ſtudents, and gradually improved by corrections and additions of ſucceſſive years. Copies of ſuch notes were in very general circulation about the College. But the very beſt of them are ſo imperfect, have been the work of perſons ſo little acquainted with general learning, and are ſo full of miſtakes, and very inadequate repreſentations of Dr. Black's ſentiments, that the executors wiſhed to prevent the unfavourable impreſſion that ſuch a publication would make of the knowledge and talents of their departed friend. Mr. Archibald Geddes, manager of the extenſive glaſs-works at Leith, who had long lived in the moſt intimate and cordial friendſhip with Dr. Black, and in whoſe attachment to his memory and reputation the executors had the greateſt confidence, mentioned me as a perſon whom their friend had thought well acquainted with chemiſtry, and who had been in the habits of intimacy with him in the period of his greateſt exertions; and, moreover, as one attached to Dr. Black by every tie of reſpect, obligation, and affection, and therefore likely to be particularly anxious to ſupport his claims to eminence, by a careful and accurate exhibition of his ſentiments and doctrines. This ſuggeſtion appeared reaſonable, and he was deſired to intimate their wiſh to me.

When the propoſal was made to me, it ſtartled me; but it pleaſed me. It was very gratifying thus to have the laſt and the beſt opportunity of paying my reſpects to the memory of my excellent friend. I was indeed attached to Dr. Black by every honourable tie. I owed him much; and the reader will, I hope, forgive me if I mention here the origin of my acquaintance with this celebrated man. It is

a piece of ſelf-indulgence, though not foreign to my preſent purpoſe, as it ſhews the early opportunities I enjoyed of acquiring a thorough knowledge of his ſtudies and character.

My acquaintance with him began at Glaſgow in 1758, I being then a ſtudent in that Univerſity; and it began in a way which marked the diſtinguiſhed amiableneſs of his diſpoſition and behaviour. It was at the houſe of one of the Profeſſors, to whom I was telling the great entertainment I had received from the lectures of Dr. Robert Dick, Profeſſor of Natural Philoſophy, and how much I admired him as a lecturer. Dr. Black joined in the commendation, and then, addreſſing himſelf to me, queſtioned me a good deal about Natural Philoſophy, ſo as to perceive what were the peculiar objects of my attention. His advices relative to my favourite ſtudy were ſo impreſſive, and given in a manner ſo unaffectedly ſerious and kind, that they are ſtill as freſh in my mind as if of yeſterday's date. I was a ſtranger to him, and not even his pupil; and he was prompted to take that pains with me, ſolely by the way in which he heard me ſpeaking of the lectures of one whom he loved and eſteemed. Gently and gracefully checking my diſpoſition to form theories, he warned me to ſuſpect all theories whatever, preſſed on me the neceſſity of improving in mathematical knowledge, and gave me Newton's Optics to read, adviſing me to make that book the model of all my ſtudies, and to reject, even without examination, every hypothetical explanation, as a mere waſte of time and ingenuity. I am conſcious that it was to theſe advices, ſo impreſſively, becauſe ſo kindly beſtowed, that I owe any ability that I may now poſſeſs for ſcientific attain-

ments: For he ſet me into a path which I fear I ſhould never have choſen for myſelf.

Our acquaintance was ſoon interrupted by my leaving college. When I returned to Glaſgow, after four years abſence, I became Dr. Black's ſcholar. He was then engaged in his ſpeculations about what he termed *latent heat*. I renewed my acquaintance with him, and with Mr. James Watt, the celebrated engineer. He had already heard two courſes of the Profeſſor's lectures, and was completely maſter of the ſubject. Dr. Black was pleaſed to admit me to the greateſt intimacy of acquaintance, and few days paſſed in which I was not in company with theſe eminent philoſophers, both of them in the prime of their ſcientific energy, and the period of their keeneſt reſearch. In ſuch ſociety, I muſt have been ſtupid not to improve; and this is the time of my life that I recollect with the greateſt ſatisfaction.

When Dr. Black was removed to Edinburgh in 1766, he recommended me to the Univerſity of Glaſgow as his ſucceſſor in the chemical lectureſhip. I exerted my utmoſt endeavours not to be altogether undeſerving of this honour; and I am happy to think that theſe endeavours obtained his approbation, of which he gave me the moſt convincing proof, by recommending me in the warmeſt terms to the Patrons of the Univerſity of Edinburgh, as well qualified to fill the Profeſſorſhip of Natural Philoſophy, vacant by the death of Mr. Ruſſel, his near relation; a recommendation which, doubtleſs, had ample weight in deciding the competition in my favour. I became Dr. Black's colleague in 1774, and ever ſince that time have been honour-

ed by his uninterrupted intimacy and friendſhip. *Si quid eſt in me ingenii, quod ſentio quam ſit exiguum, aut ſi hujuſce rei ratio aliqua, ab optimarum artium ſtudiis ac diſciplinâ profecta, a quâ ego nullum confiteor ætatis meæ tempus abhorruiſſe, earum rerum omnium hic Aulus Licinius fructum a me repetere propè ſuo jure debet. Nam quoad longiſſimè poteſt mens mea reſpicere ſpatium præteriti temporis, et pueritiæ memoriam recordari ultimam, inde uſque repetens, hunc video mihi principem, et ad ſuſcipiendam, et ad ingrediendam rationem horum ſtudiorum extitiſſe* *.

The propoſal, then, upon the part of Dr. Black's Truſtees and Executors, for charging me with this important duty to his memory, was not without ſome very reaſonable inducements. But every circumſtance which rendered this propoſal acceptable to me, contributed to point out to me another perſon, more fit for the taſk, and more likely to execute it as it ought to be done. This was Mr. Watt of Birmingham ;—attached to Dr. Black by the ſame ties, having had ſuperior opportunities of knowing the whole train of his thoughts, and far more able to do juſtice to his merits, becauſe profoundly verſed in chemical ſcience. I adviſed this choice, and Mr. Watt was applied to ; but his occupations were too inceſſant, and too ſerious, to allow him to think of ſuch an avocation.

I had no reaſon for declining the taſk, but the very powerful one of bad health, and the fear of its growing worſe, and my being thereby rendered unable to fulfil my engagements. It was, however, ſtrongly preſſed upon me ; ſo that, after ſome fear and heſitation, I agreed to the propoſal, on condition of having Mr. Watt's aſ-

* Cicero pro Archiâ Poetâ.

ſiſtance, which the Executors requeſted and obtained, as far as could be obtained by a correſpondence of letters, which Mr. Watt entered into with great cheerfulneſs and zeal.

But I had not ſufficiently weighed the burden which I had taken on my ſhoulders. I had been informed that Dr. Black had, for two or three years before his death, occupied himſelf in the reviſal of the notes of his lectures, and had brought them into very good order. Two or three of them, which I looked into, in order to form a judgment of my taſk, correſponded with this account,—and the engagement was entered into. This was in January 1800, while I was occupied with my own college duty, ſo that I could do nothing in the affair till the May following. When I then entered ſeriouſly on the taſk, I found that the notes were (with the exception of perhaps a ſcore of lectures) in the ſame imperfect condition that they had been in from the beginning, conſiſting entirely of ſingle leaves of paper in octavo, full of eraſions, interlinings, and alterations of every kind; ſo that, in many places, it was not very certain which of ſeveral notes was to be choſen. They were often in ſuch a ſtate, that I could not give them to my amanuenſis to be tranſcribed; and the only thing that could be done was for me to dictate from them. I took this method, as the only ſecurity for obtaining a fair tranſcript. This proceſs neceſſarily conſumed a great deal of time before I got to the end. It was only then that I could form a judgment of the performance; for, as I was going on, almoſt decyphering, my attention was wholly engroſſed by the lines before me, and I had ſcarcely any notion of a page of it, taken together.

I now found a difficulty of another kind. Throughout the whole ſeries of Lectures, wherever the ſubject was very plain and obvious, the manuſcript contained merely a memorandum, from which Dr. Black had lectured extempore; in many places, a reference was made to ſomething ſtanding on the table, or ſomething going forward in the furnaces. All thoſe blanks were to be filled up, before I could ſay that I had made out even a rough draught of the lectures. This was done, and then it only remained to make ſome alterations in the modes of expreſſion, to cancel alluſions to a former day's lecture, and other circumſtances of this kind, which were not ſuitable to the appearance in the form of a book. In a few places, I found myſelf conſiderably at a loſs to aſcertain the author's meaning, when the reference was very ſlight, often in a note with the pencil. I mention all theſe circumſtances, to account for the ſeeming delay in the publication. No doubt, my proceeding was ſlower than it would have been had I been in good health; but the additional delay on this account has not been conſiderable. I had the aſſiſtance of a very fair copy of notes, taken by a ſtudent, or rather manufactured by the compariſon of many ſuch notes. Copies of this kind were to be purchaſed for four or five guineas. This copy belonged to Dr. Black, and he had made many alterations and inſertions of whole pages with his own hand. It was of conſiderable ſervice to me for filling up the blanks above mentioned. Beſides the notes which Dr. Black had before him while he lectured, and which were all put into ſeparate parcels, each of which contained a lecture, there are other ſmall parcels, titled with the different articles of the courſe, and containing notes and me-

morandums of experiments, quotations from authors, ſpeculations and conjectures on intereſting facts or opinions. From theſe alſo I was frequently enabled to ſupply what Dr. Black had ſaid in the lecture.

With ſuch helps, I truſt that I have omitted nothing of any importance, and have every where expreſſed Dr. Black's ſentiments with accuracy. This is always done in his own words, except in the caſes already mentioned, where I filled up a blank in the manuſcript. Even in thoſe caſes, if the words of the above mentioned notes taken in the claſs expreſſed the ſubject with diſtinctneſs, I took them, in preference to any inſertion of my own, as probably not greatly differing from Dr. Black's diſcourſe. Where I had no ſuch help, I queſtion not but that the difference between Dr. Black's manner of expreſſing himſelf, and mine, will be perceived by the gentlemen who had the pleaſure of hearing him. I am ſenſible that his language had a perſpicuous ſimplicity which I cannot attain.

I have twice taken the liberty of making a change in the arrangement of an article. Dr. Black had conſidered the *pyrophori*, ſuch as the *phoſphorus of alum*, of *Baldwin*, the *Bolognan ſtone*, *&c.* under the ſame title with the *phoſphorus of urine*, ſolely, I preſume, becauſe they are uſually called by the ſame name, *phoſphorus*. I have placed them at the end of the article *charcoal*, becauſe they all contain charcoal, and cannot be well underſtood till the properties of ſulphur and of charcoal have been explained. For the ſame reaſon, a ſmall part of the account of the *diamond* is ſeparated from the reſt, and inſerted in the article *charcoal*. The other caſe is the account of the obſervations and experiments of M'Bride, Lane, Percival, and thoſe of Prieſtley,

Cavendiſh, Scheele, and Lavoiſier, ſubſequent to the eſtabliſhment of the theory of quicklime. (*See vol. II. page* 89, &c.) Theſe particulars ſtood in an order ſomewhat different from what is followed here; and I truſt that the change will be thought to render the account more connected and perſpicuous. Indeed, the whole of the article *Alkaline Earths* is not in a form that gave ſatisfaction to the author; and there lie among theſe papers ſeveral projects for changing both the arrangement and the manner of treating the ſubject. Dr. Black had always paid too much deference to the futile objections of Profeſſor Meyer, and the partiſans of his *acidum pingue;* and had continued to treat thoſe objections as ſtill requiring a refutation, after the theory of *acidum pingue* was altogether exploded. He gives ſufficient indication, in theſe memorandums, of the manner in which he intended to conſider the ſubject; and I ſhould willingly have made the alteration, had I not thought it too great a liberty to inſert a compoſition of my own, in the place of any thing written by Dr. Black.

The memorandums on the medicinal preparations of mercury are ſo extremely ſlight and imperfect, that, ignorant as I am of medicine and pharmacy, I could not venture to make any uſe of them. The memorandums on the chemical analyſis of animal and vegetable ſubſtances are not in condition fit for publication, not being at all accommodated to the preſent ſtate of chemical ſcience. Should it appear that this publication is favourably received by the public, an appendix will follow, in the ſame form, in which theſe two articles will be properly treated. The doctrine of fermentation (vinous, acetous, and putrefactive) will make a principal article in that work, ſeeing that they

are of the very firſt importance, both to the artiſt and to the philoſopher.

The diſcoveries which have been made within theſe laſt thirty years, and which have produced ſo great a change in chemical ſcience, were adopted by Dr. Black, and introduced into his lectures, in proportion as he ſaw them well ſupported and confirmed. This, however, taking place by degrees, has occaſioned frequent changes in his lectures, and I am not certain that he has been fully decided as to the way in which he ſhould introduce the new doctrines of combuſtion and acidification. His papers contain ſeveral plans to this purpoſe. One of them, which ſeems to have had the preference in his eſtimation, is to bring the whole gradually into view, in the hiſtory of the nitric acid, in all its various relations. This method would have been extremely perſpicuous and convincing, and had I had ſufficient materials of Dr. Black's compoſition for the beginning of this hiſtory, I ſhould perhaps have followed it out, although it would have required almoſt a new writing of the whole particular doctrines of chemiſtry. But this not having been ſupplied to me, I had not ſufficient authority for making ſuch a change. As it ſtands in theſe volumes, the doctrines are all fully explained, and ſupported by topics of argument that are quite eaſy and familiar; and this is done in a manner that gives full confidence. The only deficiency that I perceive is in the incidental way in which the compoſition of the volatile alkali is introduced; and perhaps alſo the peculiarities of the oxygenated muriatic acid. Had manganeſe been firſt conſidered merely as a ſubſtance affording vital air, the oxygenated form of the acid would

have been early eſtabliſhed, and this would have afforded ſolid arguments for ſome other abſtruſe points of doctrine.

A diſcourſe, given at the firſt meeting in each courſe of lectures, has been omitted entirely, as having little or no connection with their appearance as a book; and a ſhort hiſtory of chemiſtry has alſo been left out, as too general to be very intereſting or inſtructive.

On the whole, what follows in the text of this work, is the diſcourſe of Dr. Black; as much as poſſible in his own words, and, in every paſſage, a faithful exhibition of his ſentiments and opinions. I have added ſome notes, chiefly relative to the author's peculiar doctrines, and his claims to originality and priority; ſometimes illuſtrative of the text; and, on a few occaſions, contributing, I hope, to the reader's acquiring juſt and philoſophical notions of the ſubject. I hope that they will be found pertinent, and, in general, that my endeavours to make the labours of my venerated maſter uſeful, as he earneſtly wiſhed them, and to appear not unworthy of himſelf, will be indulgently received by a candid public, to whoſe judgment they are now ſubmitted with all becoming deference and reſpect.

When we have received much pleaſure or inſtruction from the writings of any perſon, we are apt to feel ſome attachment to the man to whom we have been thus obliged, and ſome intereſt in any thing that perſonally concerns him;—we wiſh to be better acquainted with him,—to know who he is,—what were his uſual occupations,—his fortunes,—his general manners. If we have often liſtened to him as a public ſpeaker, we cannot help forming to ourſelves a notion of his temper anddiſpoſitions from the tones of his voice, and the ex-

preſſion of his countenance; and we figure to ourſelves how this perſon would behave as a companion, a friend, or an inmate. If his public appearance has been conciliating, or eminently pleaſing, we wiſh to converſe with him in the familiar intercourſe of life, to witneſs him in his domeſtic occupations, and to ſhare in his amuſements. I think that I may venture to ſay that theſe are the wiſhes of all who have liſtened to this moſt engaging lecturer,—who have remarked the pleaſing ſmile, which began to form on his countenance, when he was about to exhibit or relate any thing that he conſidered as peculiarly intereſting. They will hold themſelves as obliged to any man who will bring them more cloſely into ſociety with this amiable perſon, by giving ſome account of his ordinary habits, his ſtudies, his purſuits, and even his fortunes.

I ſhould be happy that it were in my power to ſatisfy theſe natural wiſhes by ſome diſcriminating deſcription of Dr. Black, by ſome account of his life, his character, and manners. This would not only pleaſe, but (as I apprehend) would inſtruct, by exhibiting a fair example of what is amiable and worthy.

But I am too ſenſible *quid ferre recuſent, quid valeant humeri*, to think myſelf equal to this taſk,—to give ſuch a picture that a competent judge ſhall ſay, "This is Dr. Black." I will, however, for the ſatisfaction of his pupils, and as a ſort of duty to my departed friend, mention ſuch circumſtances of his life and ſituation as have come to my knowledge, eſpecially ſuch as have ſome relation to this publication of his official diſcourſes. For this purpoſe, I ſhall uſe the freedom to avail myſelf of the information contained in a paper read to the Royal So-

ciety of Edinburgh, (of which Dr. Black was a member), by his near relation, Dr. Adam Ferguſon, Profeſſor of Mathematics in the Univerſity, and well known in the republic of letters, by works of the very firſt rank.

Dr. Joſeph Black was born in France, on the banks of the Garonne, in the year 1728. His father, Mr. John Black, was a native of Belfaſt in Ireland, but of a Scotch family, which had been ſome time ſettled there. Mr. Black reſided, for the moſt part at Bordeaux, where he carried on the wine trade. He married a daughter of Mr. Robert Gordon, of the family of Hilhead in Aberdeenſhire, who was alſo engaged in the ſame trade at Bordeaux, where he was very ſucceſsful, and had the ſatisfaction of clearing the family eſtate in Scotland of the incumbrances which had been increaſing for ſome generations.

The mother of Dr. Black, and the mother of Mr. James Ruſſel, Profeſſor of Natural Philoſophy in the Univerſity of Edinburgh, were ſiſters; and the mother of Dr. Adam Ferguſon was their aunt, a circumſtance which was the origin, though not the cement, of a friendſhip ſubſiſting between them through life. Their oſtenſible connection became much cloſer by Dr. Ferguſon's marrying a daughter of Dr. Black's ſiſter, a young lady who had a great deal of her uncle's elegance of mind and manners.

Mr. Black of Bordeaux was a gentleman of the moſt amiable manners, candid and liberal in his ſentiments, and of no common information. Theſe features of his character, and particularly the ſtrength of his attachments, and the warmth of his heart, appear in the

ſtrongeſt manner, in a ſeries of letters to his ſon, which have been preſerved by him with the niceſt care.

So much worth had not eſcaped the diſcerning eye of the great Monteſquieu, one of the preſidents of the court of juſtice in that province. This illuſtrious magiſtrate and excellent man honoured Mr. Black with a friendſhip and intimacy altogether uncommon; of which his deſcendents are, to this day, juſtly proud. They preſerve letters, and fragments of correſpondence, between the preſident and their anceſtor, as they would titles of honour deſcending in their family. On a paper wrapped round a bundle of ſuch letters, I find the following note in the hand-writing of Dr. Black:

" My father was honoured with preſident Monteſquieu's friendſhip, " on account of his good character and his virtues. He had no am- " bition to be very rich, but was cheerful and contented, benevolent " and liberal minded, induſtrious and prudent in buſineſs, of the " ſtricteſt probity and honour, very temperate and regular in his man- " ner of life. He, and my mother, who was equally domeſtic, edu- " cated thirteen of their children, eight ſons, and five daughters, who " all grew up to be men and women, and ſettled in different places. " My mother taught her children to read Engliſh, there being no " ſchool for that purpoſe at Bordeaux."

The great partiality of the preſident Monteſquieu for the conſtitutional government of theſe kingdoms is well known. I think it not unlikely that he derived much of his information about many things peculiar to our fortunate ſituation from the Britiſh gentlemen whom he might ſee at Bordeaux. Here he could make his inquiries at his lei-

ſure, and here he was certain of being liſtened to with attention and reſpect. His ſtay in England was ſhort and hurried; too many objects were forced on his attention at once. This however was not his chief inducement to an intimate acquaintance with the family of Mr. Black. With a great ſimplicity of heart and manners, this illuſtrious perſon had a glowing ſenſe of modeſt worth. His private memorandums, lately publiſhed, ſeem truly to expreſs his genuine ſentiments and choice in the enjoyment of life. Though ſolicited by every thing that could encourage a man of honourable ambition, he quitted the capital in the height of his reputation for profeſſional talents, and even quitted his high office in the province, that he might be fully maſter of his time, and chooſe his ſociety. He lived almoſt conſtantly at his country ſeat in the neighbourhood of Bordeaux.

I am ſorry that I did not look more narrowly into the Preſident's correſpondence with Mr. Black. I can only recollect the being delighted with the impreſſion which the ſight of ſo much worth and domeſtic happineſs made on this excellent judge of human nature. Dr. Ferguſon mentions a letter, written by him, when he heard of Mr. Black's intention to leave Bordeaux. In this letter, among other expreſſions of kindneſs, are the following: "I cannot reconcile myſelf "to the thoughts of your leaving Bordeaux. I loſe the moſt agree-"able pleaſure that I had, that of ſeeing you often, and forgetting "myſelf with you." I remember alſo another letter, in which were nearly the following words: "I rejoice to hear of the good health "of all your family; and I endeavour to make your ſatisfaction ſo-"lace me for the loſs of thoſe tranquil hours which I enjoyed in the

" midſt of my friends, contemplating their happineſs and their vir-
" tues."

I could not refuſe myſelf the pleaſure of recording theſe few particulars, deſcriptive of an amiable family, which ſo warmly engaged the notice and the heart of this illuſtrious friend. But they are not without their uſe, when they ſhew the charm of domeſtic virtue, and the unſpeakable advantages of good habits in the outſet of life. Mr. Black lived to ſee that his paternal care had not been in vain.

Long before Mr. Black retired from buſineſs, his ſon Joſeph was ſent home to Belfaſt, that he might have the education of a Britiſh ſubject. This was in the year 1740, he being then twelve years of age. After the ordinary inſtruction in a grammar ſchool, &c. he was ſent, in 1746, to continue his education at college, in the Univerſity of Glaſgow. I have no account of thoſe youthful ſtudies, but I preſume that he had employed his time to good purpoſe. I infer this, partly, from ſome paſſages in his father's letters to him, expreſſive of his great ſatisfaction with the accounts which he had received from others, of his progreſs in his ſtudies; and partly, from the very perſpicuous manner in which I have heard Dr. Black ſtate the diſtinctions between the theories of ethics which had been taught at Glaſgow by Dr. Francis Hutcheſon, and, after him, by Dr. Adam Smith. Phyſical ſcience, however, had chiefly attracted his attention; and he was a favourite pupil of the profeſſor of natural philoſophy, Dr. Robert Dick, and the intimate companion of his ſon and ſucceſſor. This young profeſſor was of a character peculiarly ſuited to Dr. Black's taſte, having the cleareſt conception and ſoundeſt judgment, a manly ſteadineſs of opi-

nions and conduct, accompanied by a modeſty that was very uncommon. When he ſucceeded his father in 1751, he became the delight of his ſtudents, and was, indeed, the moſt perſpicuous and inſtructive lecturer I ever heard. Yet Dr. Black informed me that he was unhappy in the thoughts of not being ſufficiently qualified for the office, and wiſhed to reſign it. He was carried off by a fever in 1757. Dr. Black always ſpoke of him in terms of the higheſt reſpect for his talents and his great worth, and I could obſerve that he was always pleaſed when I made him the ſubject of converſation. Dr. Dick had been the choſen friend of his youth.

Being required by his father to make choice of a profeſſion, Mr. Black preferred that of medicine, as the moſt ſuited to the general habits of his ſtudies, not foreſeeing, during the happy gaiety of youth, how much he would ſuffer by anxious ſolicitude and fears in the practice of this noble art.

It was fortunate for Dr. Black that, when he began his medical ſtudies at Glaſgow, the celebrated Dr. William Cullen had juſt entered on his great career, was become conſcious of his own ſtrength, and ſaw the great unoccupied field of philoſophical chemiſtry open before him. It had been treated hitherto only as a very curious and uſeful art, which was indeed ſuſceptible of much improvement by means of rational inquiry and diſcuſſion. But Cullen ſaw in it a vaſt department of the ſcience of nature, which muſt be founded on principles as immutable as the laws of mechaniſm, and which may be one day formed into a great ſyſtem of doctrines, of various degrees of ſubordination and dependence. He was determined to attempt this mighty taſk,

and promiſed himſelf great reputation by its accompliſhment. Nor was he altogether diſappointed. He quickly ſucceeded in taking chemiſtry out of the hands of the artiſts, the metallurgiſts, and pharmaceutiſts, and exhibited it as a liberal ſcience, the ſtudy of a gentleman. He carried into his medical lectures the ſame ideas of a great ſyſtem of nature, and made his pupils perceive ſomething of that affinity by which, as Cicero finely obſerves, *all the ſciences are connected, tendering to each other a mutual illuſtration and aſſiſtance.* His pupils became zealous chemiſts as well as refined phyſiologiſts. Young Black was particularly delighted with a view which accorded ſo happily with thoſe enlarged habits of thought which he had acquired; and his great bias to this ſtudy was ſoon perceived by Dr. Cullen. No profeſſor took a more lively intereſt in the progreſs of an emulous ſtudent than Dr. Cullen. It was his delight to encourage and aſſiſt their efforts, and therefore he was not long in attaching Mr. Black to himſelf, in the moſt intimate co-operation; inſomuch, that the latter was conſidered as his aſſiſtant in all his operations; and his experiments were frequently adduced in the lecture, as good authority. Thus began a mutual confidence and friendſhip, which did honour both to the profeſſor and his pupil, and was always mentioned by the latter with gratitude and reſpect.

Our young philoſopher had laid down a very comprehenſive and ſerious plan for the conduct of his ſtudies. This appears by a number of note-books found among his papers. There are ſome in which he ſeems to have inſerted every thing as it took his fancy, in medicine, chemiſtry, juriſprudence, or matters of taſte; and I find others into

which he has transferred the ſame things, but has diſtributed them according to their ſcientific connections. In ſhort, he has kept a journal and leger of his ſtudies, and has poſted his books like a merchant. I have looked over theſe memorandums with ſome care, and have there ſeen the firſt germs of thoſe diſcoveries which have at laſt produced ſuch a complete revolution in chemical ſcience. What particularly ſtruck me, was the ſteadineſs with which he advanced in any path of knowledge,—*nulla retrorſum.* Things are inſerted for the firſt time, from ſome preſent impreſſion of their ſingularity or importance, but without any alluſion to their connections. When a thing of the ſame kind is mentioned again, there is generally a reference back to its fellow; and thus the moſt inſulated facts often acquired a connection which gave them ſcientific importance.

By theſe references I got the order in which things had occurred, (for there are very few dates) and a pretty certain knowledge of the years when he made the obſervations. For, in what I call his day-books, mention is ſometimes made of occurrences that were perfectly known to me, as I had lived in the place from my infancy, and was well acquainted with every thing that paſſed. In the oldeſt parcels of theſe notes, I find queries reſpecting the nature of cooling mixtures, and the cold produced by liquefaction; but it is not till ſome time after, I think not before the year 1752, that I can date any obſervation relative to fixed air. Yet this was the firſt of his inveſtigations that he completed, by eſtabliſhing it as a general law of nature. I do not imagine that Mr. Black's reſearches at this time (or perhaps at any time) have been keen or pertinacious. This could not accord

with the native gentleneſs of his mind; but his conceptions being diſtinct, and his judgment ſound, his progreſs in ſcientific reſearch, if ſlow, was ſteady, and his acquiſitions were ſolid. Perhaps this moderation and ſobriety of thought was his happieſt diſpoſition, and the moſt conducive to his improvement. I am convinced that it was from experience that he was led to caution his pupils ſo earneſtly to check the firſt incitements of high expectations, and never allow their fancy to be warmed by the brilliant appearance of ſome general view, which promiſed at the firſt glance to explain a multitude of phenomena.

This calmneſs of procedure appears in another circumſtance. The College Regiſters ſhew that Mr. Black might have offered himſelf as a candidate for a doctor's degree, near three years earlier than he actually did. When he was graduated in 1754, he had not availed himſelf of his medical ſtanding in the Univerſity of Glaſgow, but took the courſe preſcribed by the rules of that Univerſity.

Dr. Black went to Edinburgh, to finiſh his medical ſtudies, in 1750, or 1751. There he lived with his couſin-german, Mr. James Ruſſel, Profeſſor of Natural Philoſophy in the Univerſity. Mr. Ruſſel was really a philoſopher. No man ſaw more clearly the great ſcale of Nature, as it is diverſified by the powers of mechaniſm, chemical affinity, and the principles of growth, life, ſentiment, and intellect; and I think he was the firſt who aſcertained with ſome preciſion the true pale of Natural Philoſophy, and gave a truly philoſophical table of its various parts. He was not more enlarged in his general views, than he was ingenious in his examination of ſubordinate parts; and he was

moſt happy in the talent of preſenting things to the mind in a ſimple and familiar manner. In ſuch a ſociety, Mr. Black muſt have paſſed his time both agreeably and profitably

It was the good fortune of chemical ſcience, that at this very time, the opinions of profeſſors were divided concerning the manner in which certain lithontriptic medicines, and particularly lime-water, acted in alleviating the excruciating pains of the ſtone and gravel. The ſtudents uſually partake of ſuch differences of opinion, and are thereby animated to more ſerious ſtudy, and ſcience gains by their emulation. This was a ſubject quite ſuited to the taſte of young Mr. Black, one of Dr. Cullen's moſt zealous and intelligent chemical ſcholars. It was indeed a moſt intereſting ſubject, both to the chemiſt and the phyſician. All the medicines which were then in vogue, as ſolvents of the calculous concretion, reſembled more or leſs the *lapis infernalis*, and the common ley of the ſoap boilers, two ſubſtances ſo terribly acrimonious, that in a very ſhort time, they will reduce the firmeſt and moſt ſolid parts of the animal body to a mere pulp. Therefore, while they were powerful lithontriptics, they were hazardous medicines, if in unſkilful hands. All of them ſeem to derive their efficacy from quicklime, and this derives its power from the fire. Its wonderful property of becoming intenſely hot, and even ſometimes ignited, when moderately wetted with water, had long engaged the attention of the chemiſts. It was, therefore, very natural for them to aſcribe its power to igneous matter imbibed from the fire, retained in the lime, and communicated by it to alkalis, and other ſubſtances, which it renders ſo powerfully acrid. Hence, undoubtedly, aroſe the

denomination of *cauſticity*, given to the quality ſo induced. I ſee that Mr. Black had entertained the opinion, that cauſtic alkalis acquired igneous matter from quicklime. In one memorandum, he hints at ſome way of catching this matter as it eſcapes from lime, while it becomes mild by expoſure to the air;—but on the oppoſite blank page is written, " Nothing eſcapes,—the cup riſes conſiderably by ab-" ſorbing air." A few pages after this, he compares the loſs of weight ſuſtained by an ounce of chalk, when calcined, with its loſs when diſſolved in ſpirit of ſalt. Immediately after, a medical caſe is mentioned, which I know to have occurred in November 1752. From this it would appear, that he had before this time ſuſpected the real nature of theſe ſubſtances. He had then proſecuted his inquiry with vigour; the experiments with magneſia are ſoon mentioned.

Theſe laid open the whole myſtery, as appears by one other memorandum. " When I precipitate lime by a common alkali, there is " no effervеſcence. The air quits the alkali for the lime, but it is not " lime any longer, but c. c. c. It now effervеſces, which good lime " will not." He had now diſcovered that the terrible acrimony of thoſe powerful ſubſtances is their native property, and not any igneous matter derived from the lime, and by the lime from the fire. He had diſcovered that a cubic inch of marble conſiſted of about half its weight of pure lime, and as much air as would fill a veſſel holding ſix wine gallons, and that it was rendered taſteleſs and mild by this addition, in the ſame manner as oil of vitriol is rendered taſteleſs and mild, in the form of alabaſter, by its combination with calcareous earth.

What a multitude of important conſequences now preſent themſelves to the mind of an intelligent chemiſt and phyſician? I am inclined to think, that it was at this time that the animating hope of ſcientific reputation firſt dawned on the mind of this young philoſopher. His experiments on the lithontriptic medicines, by ſhewing him the true nature of quick-lime, had in one glance ſhewed him what cauſticity is, and to what ſubſtances it belongs, and how to induce it, or remove it, or direct its activity at pleaſure. This was a ſubject even more intereſting to the phyſicians than to the chemiſt. It had divided the opinions in both departments, and given riſe to many myſterious notions concerning the nature of fire (and favourites, becauſe myſterious) derived from the very remarkable properties of quicklime. Our notions are now altogether reverſed. Lime imparts nothing; it only removes from ſubſtances, naturally cauſtic, that air which renders them mild; and by this addition it becomes mild or inactive. Theſe myſterious notions are now exploded as mere fancies; and great ſimplicity is now perceived in thoſe operations of nature, which formerly appeared very intricate and abſtruſe. And it is evident that the practice of phyſic muſt derive unſpeakable advantage from all this information.

It is ſurely a dull mind that will not be animated by ſuch a proſpect. I preſume that Mr. Black felt its genial influence; and I ſuppoſe that having fixed on this for the ſubject of his inaugural eſſay, he deferred application for a degree, till his doctrine ſhould be eſtabliſhed beyond the poſſibility of contradiction, by a train of deciſive

experiments. Thus did an honourable ambition happily accord with his native moderation of character.

The inaugural eſſay, and the preciſe time of its appearance in public, were fortunate circumſtances for ſcience. At this very time, Dr. Cullen was removed to Edinburgh, and there was a vacancy in the chemical chair at Glaſgow. On whom could it be beſtowed with ſo much propriety, as on ſuch an alumnus of the Univerſity,—on one who had diſtinguiſhed himſelf, both as a chemiſt, and as an excellent reaſoner? For I heſitate not to ſay, that excepting the optics of Newton, there is not a finer model for philoſophical inveſtigation, than the eſſay on magneſia and quicklime. He was appointed profeſſor of anatomy and lecturer on chemiſtry in the Univerſity of Glaſgow, in 1756. Had this vacancy not happened, it might probably have been the lot of Dr. Black to practiſe medicine in ſome provincial town of Britain or Ireland; and thus occupied, the ſerious concern which he took in the caſes under his treatment would have abſorbed his whole attention, and might perhaps have overpowered his feeble conſtitution. It was therefore equally fortunate for himſelf and for the public, that a ſituation now preſented itſelf, which allowed him to dedicate his talents chiefly to the cultivation of chemiſtry, his favourite ſcience.

Here I muſt not omit a circumſtance told me by his brother. When Dr. Black took his degree in medicine, he ſent ſome copies of his eſſay to his father in Bordeaux. A copy was given by the old gentleman to his friend the preſident Monteſquieu, who, after a few days, called on Mr. Black and ſaid to him, "Mr. Black, my very good

" friend, I rejoice with you; your ſon will be the honour of your " name and of your family." That ſagacious philoſopher ſaw, with the firſt glance, the door opened to a field of reſearch, altogether novel, and of unknown extent. What could be more ſingular than to find ſo ſubtile a ſubſtance as air exiſting in the form of a hard ſtone, and its preſence accompanied by ſuch a change in the properties of that ſtone? What bounds could reaſonably be ſet to the imagination, in ſuppoſing that other aëreal fluids, as remarkable in their properties, might exiſt in a ſolid form in many other bodies, which at preſent attract no notice, becauſe of our ignorance of their nature and their compoſition? Here was a new inſtrument put into our hands, and a new mode of inveſtigation ſuggeſted; and it appeared unqueſtionable that many and great diſcoveries muſt be made in this new track of inquiry. Indeed, I have often wondered that Dr. Black was not more incited to proceed in this track, which he had ſo clearly laid open. This muſt have proceeded from the multiplicity of new duties which crowded on him at once, in conſequence of his department in the Univerſity of Glaſgow. His firſt appointment was to the profeſſorſhip of anatomy, and the lectureſhip on chemiſtry. He did not conſider himſelf as ſo well qualified to be uſeful in the former branch of medical ſtudy, but was determined to do his utmoſt. Soon after, however, he made arrangements with the profeſſor of medicine, and, with the concurrence of the Univerſity, the profeſſors exchanged their taſks.

His lectures, therefore, on the Inſtitutes of Medicine, were his chief taſk. They gave the greateſt ſatisfaction, by their perſpicuity and ſimplicity, and by the cautious moderation of all his general

doctrines. It required, however, all this perſpicuity, and all this neatneſs in the manner of exhibiting ſimple truths, to create a reliſh for this great moderation and caution, after the brilliant proſpects of ſyſtematic knowledge, to which the ſtudents had been accuſtomed from the Doctor's celebrated predeceſſor. But Dr. Black had no wiſh to form a medical ſchool, which ſhould be diſtinguiſhed by ſome all-comprehending doctrine. He contented himſelf with giving a clear and ſyſtematic account of as much of phyſiology as he thought founded on good principles, and a ſhort ſketch of ſuch general doctrines as were maintained by eminent authors, but perhaps on a leſs firm foundation. Without this, he ſaid that his ſtudents could not read their writings, which, in other reſpects, were highly valuable. He then endeavoured to deduce a few canons of medical practice; and concluded with certain rules, founded on ſucceſsful practice only, but not ſo deducible from the previouſly delivered principles of phyſiology; obſerving that we ſhould not deſpair of being able, on ſome future day, to proceed in the oppoſite direction, deducing the firſt principles entirely from the practice. It does not appear, however, that he had ever ſatisfied himſelf with his method of treating thoſe ſubjects. He did not encourage converſation on thoſe topics; and there are no remains of his medical lectures to be found among his papers. I owe the account now given of them to a reſpectable ſurgeon in Glaſgow, who attended theſe lectures in the two laſt years of his Profeſſorſhip in that Univerſity.

My acquaintance with Dr. Black gave me full opportunity of ſeeing, from his extreme anxiety about his patients, that he was deeply

impreſſed with a ſenſe of the immenſe importance of the healing art. I cannot doubt, therefore, but that his thoughts were much and moſt ſeriouſly employed on his medical lectures; and I am confident that the leſs he was ſatisfied with the limited portion of knowledge he could communicate to his ſtudents, the more earneſtly would he ſtrive to increaſe it.

It is in this way that I account to myſelf for the remarkable fact, that, although Dr. Black had opened ſuch a new and boundleſs field of chemical reſearch, which promiſed ſo much, both of knowledge and of fame, and in which we ſee, by the progreſs of ſome very ſlovenly adventurers, that it was extremely eaſy to diſcover objects, both new, and wonderful, and important; that notwithſtanding all this, he did not immediately engage with ardour and perſeverance in this race of diſcovery and of honour. No doubt, his duty as Profeſſor of Chemiſtry forced him to divide his attention, and probably other tracks of chemical inquiry might alſo hold out very tempting objects. But ſtill nothing can more clearly ſhew Dr. Black's calm and unambitious character.

But all this was very unfortunate, in my opinion, for the world. What a difference there would have been between the patient, judicious, and progreſſive inveſtigation of Dr. Black, and the haſty, wavering, and often ſlovenly experimenting of ſome manufacturers in ſcience, whoſe wiſh to get firſt to market with every thing was repreſented by them as proceeding from public ſpirit, while the endeavours of others to correct their own errors, to arrange and methodize their materials, and thus to advance ſecurely, though ſlowly, in the great

path of philoſophical diſcovery, was attributed to a narrow-minded pride, or the ſelfiſh vanity of being accounted the author of a ſyſtem. But, *eſt modus in rebus.*—It muſt be owned that Dr. Black was too little animated by his own ſucceſs,—too inſenſible to the real value of literary fame, and to the notice taken by the public of his diſcoveries, and not ſufficiently excited to the vigorous proſecution of them. His theory of quicklime and cauſticity was ſoon known to the German chemiſts, and met with ſtrong oppoſition. Myſterious doctrines concerning the intimate nature of fire were very prevalent in the German ſchools, and were very various. Their notions of the cauſticity of alkaline ſubſtances always involved ſome of theſe doctrines. This gave riſe, therefore, to a variety of objections to a doctrine which took this matter out of the hands of the pyrologiſts altogether. The moſt formidable opponent was Dr. Meyer of Oſnaburg, who had publiſhed a conſiderable volume on quicklime, in which he profeſſed to explain all the phenomena by the action of an *acidum pingue*, formed in the lime during calcination, and conſiſting of igneous matter in a certain inexplicable combination with other ſubſtances. There is ſcarcely to be ſeen a book in which there is ſuch a number of injudicious experiments, and unſkilful attempts to reaſon from them. Yet this performance of Profeſſor Meyer gave Dr. Black conſiderable uneaſineſs; and, for ſeveral years, he was at the pains to refute all his arguments, and all his objections to the theory given by himſelf, but without adding a ſingle experiment to thoſe by which he had already eſtabliſhed it. Yet the obvious and ſimple experiment, of making the matter expelled from marble by a calcining

heat paſs into a ſolution of cauſtic alkali, and render it effervescent and mild, would have ended all diſputes. This was done by Jaquin, at Vienna, in 1767, and at once ſilenced all the German chemiſts, as the experiment of Well, in which he calcined marble by a burning glaſs, put an end to Meyer's theory. But Dr. Black always expreſſed a diſlike to the bringing forward a multitude of experiments of the ſame kind, ſaying that he felt his own confidence more forcibly won by one judicious and ſimple experiment than by any number of examples of inferior evidence.

Dr. Black's reception at Glaſgow by the Univerſity was in the higheſt degree encouraging. His former conduct there as a ſtudent had not only done him credit in his claſſes, but had conciliated the affection of the Profeſſors to a very great degree. When he returned to his Alma Mater Academia as a Profeſſor, he was immediately connected in the ſtricteſt friendſhip with the celebrated Dr. Adam Smith; a friendſhip which became more and more intimate and confidential through the whole of their lives. A certain ſimplicity of character, with an incorruptible integrity, which was acutely ſenſible to the ſmalleſt indelicacy or incorrectneſs, was inſtantly ſeen by each of theſe friends in the character of the other, and riveted the band of their union. Dr. Smith uſed to ſay that no man had leſs nonſenſe in his head than Dr. Black; and he often acknowledged himſelf obliged to him for ſetting him right in his judgment of character, confeſſing that he himſelf was apt to form his opinion too generally from a ſingle feature. Indeed, were I to ſay what intellectual talent Dr. Black poſſeſſed in the moſt uncommon degree, I think

I ſhould ſay that it was his judgment of human character, and a talent which he had of expreſſing his opinion in a ſingle ſhort phraſe, which fixed it in the mind, never to be forgotten. Dr. Smith's pictures of men had not always this preciſe ſimilitude, he being more apt to decide haſtily of character; and he was not unfrequently miſtaken in the judgments he formed on a ſhort acquaintance.

When I returned to College in 1763, I found Dr. Black in high reputation as a profeſſor, and a favourite phyſician of that large and active city. Indeed his ſweetneſs of manner, which the dulleſt eye muſt have perceived to be free from all ſtudious endeavour to pleaſe, and the evident concern which he took in the caſes under his care, could not but make him a moſt welcome viſitor in every family. His countenance, at that time of life, was equally engaging as his manners were attractive, ſo that I do not wonder that, in the general popularity of his character, he was in particular a favourite with the ladies. I could not but remark that they regarded themſelves as honoured by the attentions of Dr. Black; for theſe were not indiſcriminately beſtowed, but excluſively paid to thoſe who evinced a ſuperiority in mental accompliſhments, or propriety of demeanour, and in grace and elegance of manners.

But I am forgetting the profeſſor and philoſopher. It was at this time, between the years 1759 and 1763, that he brought to maturity thoſe ſpeculations concerning the combination of heat or fire with the ſubſtance of tangible matter, which had long occupied his thoughts occaſionally. The ſimple experiments and familiar obſerva-

tions by which he demonſtrated the fixation (I may call it) of heat in bodies when it melts or evaporates them, render the inference ſo palpable and obvious, that one is diſpoſed to wonder that it had not been made long before. But it is really not ſo obvious, and it requires attentive reflection, to conceive diſtinctly the procedure of nature. When I lift a piece of wood out of ſome box or veſſel, where every thing has been kept extremely cold, I feel it cold in my hand. If I lift out of the ſame box a piece of lead, it feels colder ſtill; and a piece of ice from the ſame place feels colder than either. The firſt ſuggeſtion of ſenſe is, that I *receive* cold from the wood;—more from the lead;—and that the ice proves a ſource of cold till it be all melted. But the perſon who is habituated to the conſideration of things of this nature makes an inference which is preciſely the contrary to all this. Such a perſon infers that the wood *takes* a little heat from his hand, and is ſoon warmed ſo much as to take no more. The lead takes more heat from him before it be as much ſatiated; and the ice continues to rob him of heat as faſt as in the firſt moment, and therefore feels equally cold till all be melted. Dr. Black made this inference. He had alſo ſome vague notion that the heat ſo received by the ice, during its converſion into water, was not loſt, but was contained in the water. His chief inducement to think ſo, was a curious obſervation of Fahrenheit, recorded by Dr. Boerhaave, namely, that water would ſometimes grow conſiderably colder than melting ſnow, without freezing, and would freeze in a moment, if diſturbed, and in the act of freezing emitted many degrees of heat.

But how was this conjecture to be confirmed or refuted? Dr.

Black hoped to do this by comparing the time of raiſing a pound of water one degree in its temperature, with the time required for melting a pound of ice, both being ſuppoſed to receive the heat equally faſt. And on the other hand, by comparing the time of depreſſing the temperature of a pound of water one degree, with the time neceſſary for freezing it completely, he ſhould learn how much heat emerged during the congelation. If the conjecture be juſt, as much heat muſt be obſerved to come out of the pound of water in freezing as were loſt in melting a pound of ice. This thought occurred to him in the ſummer ſeaſon; and as there was no ice-houſe then in Glaſgow, he waited with impatience for the winter; and in December 1761, he made the deciſive experiment, in a large hall adjoining to his college rooms, expending on the ice during its liquefaction, and obtaining from the water during its congelation, as much heat as would have raiſed the water ſomewhat more than 140 degrees in its temperature.

But I muſt obſerve here, that this experiment, ſo anxiouſly longed for by Dr. Black, only ſerved to inform him *how much* heat was thus abſorbed by the ice, retained by the water while fluid, and emitted by it in the act of freezing. But he had already full conviction of the truth of the doctrine, by reflecting on the obſervations of every day of froſt or thaw. Since a fine winter day of ſunſhine did not at once clear the hills of ſnow, nor a froſty night ſuddenly cover the ponds with a thick cake of ice, Dr. Black was already convinced that much heat was abſorbed and fixed in the water which ſlowly trickled from the wreaths of ſnow; and on the other hand, that much heat emerged from it

while it was as ſlowly changing into ice. For, during a thaw, a thermometer will always ſink when removed from the air into melting ſnow; and during ſevere froſt, it will riſe when plunged into freezing water. Therefore, in the firſt caſe, the ſnow is receiving heat, and in the laſt, the water is allowing it to emerge again. Theſe were moſt unqueſtionable inferences, from obſervations the moſt familiar; and they now appear moſt obvious and eaſy: Yet, before Dr. Black, no perſon ſeems to have made them. Fahrenheit, Boerhaave, Mairan, De Luc, and all the inquiſitive meteorologiſts of the two preceding centuries, though inceſſantly contemplating and employing the ſame facts in their diſquiſitions, never mention having had ſuch a thought; nor is a trace of it to be ſeen in the laborious collections of that unwearied compiler, Profeſſor Muſchenbroeck. It is the undivided property of my ingenious and acute preceptor.

Philoſophers had long been accuſtomed to conſider the thermometer as the ſureſt means for detecting the preſence of heat or fire in bodies, and they diſtruſted all others. Yet this inſtrument gave no indication of the preſence of theſe 145 degrees of heat in the water. Dr. Black therefore ſaid that the heat is *concealed* in the water—*latet;* and he briefly expreſſed this *fact*, by calling it concealed or *latent heat.* The epithet expreſſed purely and accurately the very circumſtance he wiſhed to expreſs, and he could not have pitched on one more proper. Yet even this unexceptionable epithet was ſometimes miſunderſtood; and latent heat was ſpoken of as ſomething different from other heat. But this proceeded from mere inattention.

Dr. Black having eſtabliſhed this diſcovery in the moſt incontrovert-

ible manner, by ſimple and deciſive experiments, drew up an account of the whole inveſtigation, and the doctrine which he founded on it, and read it to a literary ſociety which met every Friday in the Faculty-room of the college, conſiſting of the members of the Univerſity, and ſeveral gentlemen of the city, who had a reliſh for philoſophy and literature. This was done April 23. 1762, as appears by the regiſters of the ſociety.

Dr. Black quickly perceived the vaſt importance of this diſcovery; and took a pleaſure in laying before his ſtudents a view of the extenſive and beneficial effects of this habitude of heat in the economy of nature. He made them remark how by this means there was accumulated, during the ſummer ſeaſon, a vaſt magazine of heat, which, by gradually emerging, during congelation, from the water which covers the face of the earth, ſerves to temper the deadly cold of winter. Were it not for this quantity of heat, amounting to 145 degrees, which emerges from every particle of water as it freezes, and which diffuſes itſelf through the atmoſphere, the ſun would no ſooner go a few degrees to the ſouth of the equator, than we ſhould feel all the horrors of winter.

His thoughts on this combination of heat were not confined to the ſimple caſe of water, but extended to every phenomenon of congelation and liquefaction, not even excepting the changes which are effected by the functions of animal and vegetable life. He conceived the accretion of ſolid matter as a ſource of a part at leaſt of the warmth of animals.

His thoughts running in this manner over every phenomenon of

compoſition and reſolution of heat, he had long found reaſon to ſuſpect the legitimacy of the meaſures of heat given by the thermometer. He had made experiments on ſome bodies which change their mode of aggregation from ſolid to fluid, and the contrary, not all at once, but by imperceptible degrees; and had found that, in the whole of this change, they were abſorbing heat. Reſin and ſealing wax are very clear examples of this. A thermometer plunged into reſin in very thin fuſion, and compared with another ſtanding in oil in the ſame veſſel, ſhewed, from the very beginning, a very different progreſs of refrigeration. We are not certain that ſomething of the ſame kind does not happen in water and other fluids. They may have different degrees of fluidity in different temperatures, although we have no way of diſcovering them, ſo fluid do they ſeem in all temperatures.

Such were the ſurmiſes which made Dr. Black think it neceſſary to examine the ſcale of the thermometer, in order to learn whether equal differences of expanſion correſponded to equal additions or abſtractions of heat. He made a ſeries of experiments on this ſubject, and read an account of their reſult in the literary ſociety above mentioned, on the 28th of March 1760.

The reſult of this inquiry was, that equal additions or abſtractions of heat produced equal variations of bulk in the liquor of the thermometers employed by him, and therefore that the ſcale of expanſion was alſo a ſcale of heat. Dr. Black did not know, at that time, that the celebrated mathematician, Dr. Brooke Taylor, author of the *Method of Increments*, had had the ſame doubts reſpecting the thermometric ſcale, and had examined it by the very ſame experiments.

Theſe ſurmiſes and doubts about the truth of the thermometrical indications, aroſe entirely from the notion which was floating in his mind about *liquefying heat*, and from the partial and incomplete nature of his occaſional experiments on melting and congealing bodies. Much about the ſame time, Mr. De Luc entertained ſimilar ſuſpicions; but they proceeded from conſiderations altogether different; from doubts about the equableneſs of expanſion by equal variations of temperature; in ſhort, from the ſame doubts that had occurred long before to Brooke Taylor, and to Rhenaldini. Theſe were the thoughts of a philoſopher intereſted only in an inſtrument of reſearch. But the reſult of his ſcrupulous examination of thermometers was moſt unexpected and important; for, without this information, naturaliſts were liable to enormous miſtakes in their judgments of temperature. Dr. Black's ſurmiſes about the thermometric ſcale were thoſe of a chemiſt, ſtudying the nature of fluidity.

There is ſuch an analogy between the ceſſation of thermometric expanſion, during the liquefaction of ice, and during the converſion of water into ſteam, that Dr. Black had no ſooner explained the firſt of thoſe anomalies, than he felt in his own mind that all his former conjectures about a variety of phenomena in the boiling, and even in the gentle evaporation of fluids, were well founded; and he was perſuaded that in the ſame manner as ice, in liquefaction, requires the combination of a great quantity of heat, in order to form water, ſo water, in order to its converſion into ſteam, alſo requires another combination with heat, in an unknown proportion. When he conſidered the ſlow production of ſteam, notwithſtanding the continued

heat of glowing fuel in contact with the veſſel,—the ſcalding power of ſteam,—and the great heat raiſed in the refrigeratory of a ſtill,—he was ſo much convinced of the perfect ſimilarity of Nature's procedure in both caſes, that he taught this doctrine in his lectures in 1761, before he had made a ſingle experiment on the ſubject; and he explained with great felicity of argument, many phenomena of nature which reſult from this *vaporific combination of heat*. This muſt not be conſidered as unwarrantable or hypothetical. It reſulted from a careful ſtudy of thoſe facts which the operations of nature continually preſented to him. He ſaw no occaſion for more experience for eſtabliſhing the fundamental propoſition. I have ſome notes taken in the claſs this ſeſſion, by a nobleman eminent for his ſcience and learning, by which it appears that Dr. Black had brought his thoughts on this ſubject to *full maturity*, and that nothing was wanting but a ſet of plain experiments, to aſcertain the *preciſe quantity* of heat which was combined in ſteam, in a ſtate not indicated by a thermometer, and therefore latent, in the ſame ſenſe that the *liquefying heat* is latent in water.

Whoever thinks ſeriouſly on the many intereſting objects which chemiſtry preſents on every hand to the man of philoſophical curioſity,—how many things appear, of which we can give no account, and yet are of extenſive influence in many departments of the ſcience,—and who reflects on the feelings of an ingenuous and honourable mind, engaged by duty to give inſtruction on all theſe particulars,—will not wonder that Dr. Black did not immediately ſupport his doctrine by an apparatus of experiments, which ſhould fix every point, and

leave nothing to be added by others to a diſcovery ſo new, ſo curious, ſo important, and ſo intimately connected with his theory of liquefaction. But Dr. Black was not a trader in ſcience,—nor had he any ſtrong incitement from literary ambition, to make him neglect, either his ſyſtem of lectures, every one of which required his earneſt ſtudy, or his patients, whoſe caſes at all times filled him with anxiety and ſolicitude. It was late before he had made ſuch experiments as ſatisfied him, in reſpect to the preciſe quantity of the heat latent in ſteam,—not till the ſummer of 1764. But he had not reſted all this while ſatisfied with mere conjectures. He thought that what we may obſerve every day is ſufficient for ſettling the main queſtions; for we know that the temperature of a fluid riſes by every addition of heat, till it begins to boil,—after which it riſes no more, let it boil ever ſo violently. We know that ſteam, though ſo powerfully ſcalding, is no hotter by a thermometer than boiling water. Therefore the heat which enters the water while boiling, is either loſt altogether, or is concealed from the thermometer,—*latent in the ſteam.* And laſtly, the obſerved ſcalding power of ſteam, and the heat which it imparts to the worm tub of a ſtill, are ſufficient proofs that this heat is really contained in the ſteam, and may be brought out of it again by reconverting it into boiling hot water. Dr. Black had verified all this, and narrated his experiments in his lectures. The experiments were of the moſt ſimple kind, and the inference from them moſt obvious and ſatisfactory. He obſerved that whatever time was employed to heat water from its ordinary temperature (about 50°), to the boiling temperature (212°), the ſame fire muſt be applied five times as long, in order to convert it

all into ſteam. Hence he might infer that the ſteam had carried off 810 degrees of heat. He found, on the other hand, that a pound of water paſſing along the worm of a ſtill in the form of ſteam, communicated 20 degrees of heat to 40 pounds of water in the worm tub. Hence he inferred that the ſteam had given out 800 degrees of heat.

Fortunately for Dr. Black, and for the world, he had now gotten a pupil who was as keenly intereſted in this ſcientific queſtion as the Profeſſor. This was Mr. James Watt, then employed in fitting up the inſtruments in the M'Farlane Obſervatory of the Univerſity; a philoſopher, in the moſt exalted ſenſe of the word, who never could be ſatisfied with a conjectural knowledge of any ſubject, and who grudged no labour or ſtudy to acquire certainty in his reſearches. He chanced to have in his hands, for repairs, a model of Newcomen's ſteam-engine, belonging to the Natural Philoſophy Claſs, and was delighted with the opportunity which this ſmall machine gave him for trying experiments connected with the theory of ebullition, which he had juſt learned from Dr. Black. Theſe he proſecuted in a moſt happy train of ſucceſs, and did not ſtop, till his ſteam engine was rendered more like the moſt docile of animals, than a frame of lifeleſs matter; ſo that, while its power is competent to the lifting a houſe from its place, a child of ten years old ſhall, with a touch of his hand, make it go faſt or ſlow, forwards or backwards, and act either powerfully or feebly. This gentleman, attached to Dr. Black by every tie of reſpect, eſteem, and affection, ſupplied him with proofs and illuſtrations in abundance, of all the points on which the Profeſſor wanted information. Theſe were always recited in the claſs, with the moſt

cordial acknowledgment of obligation to Mr. Watt. Mr. Irvine alſo, a young ſtudent of medicine, of a quick apprehenſion, ſound underſtanding, and particularly diſpoſed to conſider every thing mathematically, was at this time a hearer of Dr. Black's lectures, and greatly captivated with chemical ſcience. He engaged, with great pleaſure and zeal, in all examinations which ſeemed to intereſt the Profeſſor, and particularly ſuch as would admit of mathematical conſideration ;—thermometrical experiments on the ſcale of heat ;—on the connection between expanſion and variation of temperature ;—on the meaſures of heat, &c. ;—all theſe were fields of reſearch altogether to his mind. He ſupplied Dr. Black with a vaſt number of experiments on the equilibrium of heat, on the ſpecific heats of different ſubſtances, and on the continued abſorption and fixation of heat by glaſs, ſealing-wax, reſin, and other ſubſtances, which gradually become more fluid. The regiſter of theſe experiments are in my poſſeſſion, and are ſimilar to thoſe which now fill many pages of the Memoires of the foreign Academies. I think it my duty now to call upon his ſon, who, I am informed, inherits much of his father's philoſophical ſpirit and ingenuity, to look over his papers, and ſee whether any of them have been put into a ſtate fit for public view, being confident that the ſtudies of ſuch a man as Dr. Irvine muſt have been extremely ingenious and important.

It was with Mr. Irvine's aſſiſtance that Dr. Black made his firſt experiments for meaſuring the latent heat of ſteam. He found it to be as much as would raiſe ſeven or eight hundred times as much water, one degree in its temperature by Fahrenheit's ſcale ; which fact Dr.

Black expreſſed by ſaying that ſteam contained 700 or 800 degrees of heat latent in it. Afterwards, with the aſſiſtance of Mr. Watt, and better apparatus, he found that the latent heat of ſteam was not leſs than 850, and ſometimes very conſiderably exceeded this, being ſo much the more as the preſſure of the atmoſphere diminiſhed.

Thus was eſtabliſhed another law of nature, of moſt extenſive and important influence in the train of changes that are going on around us. Here we obſerve another combination of heat or fire, the mighty agent by whoſe operation all theſe changes are effected. Heat, or the cauſe of heat, ſeems now to put on a real form, and is no longer liable to be conſidered as a mere condition or ſtate, into which other matter may be brought; as noiſe or ſound is known to indicate merely a certain undulating or tremulous motion of air, or other elaſtic matter. But we now ſee heat ſuſceptible of fixation, of being accumulated in bodies, and, as it were, laid by, till we have occaſion for it; and we are as certain of getting the ſtored-up heat out of the ſteam or the water, by changing them into water or ice, as we are certain of getting out of our drawer the things we laid up in them.

The influence of this laſt combination of heat is much more extenſive than appears in the experiments by which its reality was eſtabliſhed. Dr. Black diſcovered that this abſorption and accumulation takes place, not only in the boiling of all fluids, and all converſions of matter into ſtrongly elaſtic ſteams, but alſo in every caſe of evaporation, even the moſt gentle and unperceived. When the hand is dipped into warm water, and then held up in the air till the film of water adhering to it is dried off, we feel it remarkably colder

than the other hand, expoſed to the ſame air. If we dip one finger into a glaſs of water, and another of the ſame hand into a glaſs of ſtrong ſpirit of wine, and hold up the hand in the air, the finger dipped into the ſpirit is the firſt dry, and till it be dry, it feels remarkably colder than the other; but now, the other continues the colder of the two, till it alſo be perfectly dry.

Here we can alſo trace another magnificent train of changes, which are nicely accommodated to the wants of the inhabitants of this globe. In the equatorial regions, the oppreſſive heat of the ſun is prevented from a deſtructive accumulation by copious evaporation. The waters, ſtored with their vaporific heat, are thus carried aloft into the atmoſphere, till the rareſt of the vapour reaches the very cold regions of the air, which immediately forms a ſmall portion of it into a fleecy cloud. This alſo further tempers the ſcorching heat by its opacity, performing the acceptable office of a ſcreen. From thence, the clouds are carried to the inland countries, to form the ſources in the mountains, which are to ſupply the numberleſs ſtreams that water the fields. And, by the ſteady operation of cauſes which are tolerably uniform, the greater part of the vapours paſs on to the circumpolar regions, there to deſcend in rains and dews; and in this beneficent converſion into rain by the cold of thoſe regions, each particle of ſteam gives up the 700 or 800 degrees of heat which were latent in it. Theſe are immediately diffuſed, and ſoften the rigour of thoſe leſs comfortable climates.

Surely then, theſe two chemical laws of nature are curious, of extenſive influence, and of mighty importance. The diſcovery, and the

ſatisfactory eſtabliſhment of them, were titles to fame and honour, and the name of Dr. Black ſhould have now been familiar among the philoſophers of Europe. About this he gave himſelf little concern, and was perfectly ſatisfied when he ſaw that his pupils underſtood the doctrine as delivered in the lectures. One thing indeed gave him much ſatisfaction. Mr. Watt, whoſe worth of heart was now as well known to Dr. Black as his excellent underſtanding, and who was become his intimate friend, had obtained his Majeſty's patent for the improvement which he had made on the ſteam-engine, by his judicious application of Dr. Black's inſtructions, and was now in the ſtraight road to riches and fame. Dr. Black would ſcarcely have been more gratified, had thoſe advantages accrued to himſelf. Their joint ſtudies had brought to light many unnoticed properties of vapours; and I believe that both the friends conſidered this period of ſucceſsful inveſtigation as among the moſt fertile of enjoyment of any part of their lives. I had the pleaſure of witneſſing ſome of their inquiries, and ſharing in the knowledge reſulting from them; and to me alſo this period is matter of the moſt pleaſing recollection.

Meantime, Mr. Watt's engine became very generally known through Europe. Its immenſe ſuperiority, in reſpect of power and economy, offered to the buſy part of ſociety a moſt certain and powerful firſt mover for all machinery; and thus attracted the attention of all thoſe engaged in the great buſineſs of making money. It was this, more than all the love of knowledge, ſo boldly claimed by the eighteenth century, that ſpread the knowledge of the doctrine of latent heat, and the name of Dr. Black. Chemiſts, mechanicians, me-

teorologiſts, manufacturers, all took an intereſt in it; and publications and plagiariſms multiplied apace. Theſe were totally diſregarded by the unambitious author, who, in the mean while, was happy in the ſucceſs of his friend, and in the thoughts of having exerted his own talents ſo uſefully for the public.

Dr. Black continued in the univerſity of Glaſgow from 1756 to 1766, much reſpected as an eminent profeſſor, much employed as an able and moſt attentive phyſician, and much beloved as an amiable and accompliſhed gentleman, and happy in the enjoyment of a ſmall but a ſelect ſociety of friends. Often, however, have I ſeen how oppreſſive his medical duties were to his ſpirits, when he ſaw that all his efforts did not alleviate the ſufferings of the diſtreſſed. When his dear friend Dr. Dick was carried off, Dr. Black's diſtreſs was indeed exceedingly great, particularly, as he thought that another mode of treatment might have been more ſucceſsful.

In the mean time, his reputation as a chemical philoſopher was every day increaſing; and pupils from foreign countries carried home with them the peculiar doctrines of his courſes; and fixed air and latent heat began to be ſpoken of among the naturaliſts on the Continent. The progreſs however of this diffuſion of knowledge muſt have been ſlow, had things continued in the ſame train. But in 1766 Dr. Cullen, chemical profeſſor in Edinburgh, was appointed profeſſor of medicine, and thus a vacancy was made in the chemical chair of that Univerſity. There was but one wiſh with reſpect to a ſucceſſor. Indeed, when the vacancy happened in 1756, by the death of Dr. Plummer, the reputation of young Black was ſo high, as a perſon not only

ingenious and inventive, but ſingularly correct and logical in his manner of thinking and writing, that, had the choice depended on the univerſity, the newly graduated phyſician would have been profeſſor of chemiſtry. He had now, in 1766, greatly added to his claim of merit, by his more important diſcovery of the procedure of nature in producing fluidity and vapour; and he had acquired the high eſteem of all, by the ſingular moderation and ſcrupulous caution which marked all his reſearches.

Such a man was of the higheſt value to a celebrated ſeminary of learning. Ingenious men, of a fertile and lively imagination, are but too apt to give a looſe to their fancy, in forming wide-graſping theories, and dreſſing them out in ſpecious attire. The young ſtudent, ardent and credulous, is dazzled by what appears a ſtrong and wide-ſpreading light, not remarking that perhaps it is not the natural emanation from a luminary, but is artificially collected by mirrors and glaſſes; or that what he takes for real objects are only the ſhadowy repreſentations by a magic lanthorn. To this, in a great meaſure, may we aſcribe the continual flux of theory which may be obſerved in all univerſities. Yet the conſequences to ſcience are moſt unfortunate. Not only do the precious years of youth and of mental energy paſs on without ſolid inſtruction, but alſo the moſt unfortunate of all habits is acquired, that of conſidering the extenſive and plauſible application of a theory to the explanation of phenomena as a valid proof of its truth. But, on the other hand, the lectures of ſuch a teacher as Dr. Black, never permitting this play of fancy, and even rarely introducing conjecture, would be ſafe leſſons for ingenuous youth. The

affirmations of the profeſſor may be truſted as matter of experience, and the ſtudent will acquire betimes the habit of never proceeding, in reſearch of any kind, without ſounding the channel as he advances.

Dr. Black was appointed to the chemical chair at Edinburgh, to the general ſatisfaction of the public; but the univerſity of Glaſgow thence ſuſtained an irreparable loſs. In this new ſcene, his talents were more conſpicuous, and more extenſively uſeful. He ſaw this; and while he could not but be highly gratified by the great concourſe of pupils, which the reputation of the medical colleges of Edinburgh brought to his lectures, his mind was forcibly impreſſed by the importance of his duties as their teacher. This had an effect, of which it is difficult to ſay whether it has been fortunate for the public or not. Dr. Black now formed the firm reſolution of directing his whole ſtudy to the improvement of his ſcholars in the *elementary* knowledge of chemiſtry. He ſaw too many of them with a very ſcanty ſtock of previous learning. He had many from the workſhop of the manufacturer, who had none at all; and he ſaw that the number of ſuch hearers muſt increaſe with the increaſing activity and proſperity of the country: And theſe appeared to him as by no means the leaſt important part of his auditory. To engage the attention of ſuch pupils, and to be perfectly underſtood by the moſt illiterate, was therefore conſidered by Dr. Black as his moſt ſacred duty. Plain doctrines, therefore, taught in the plaineſt manner, muſt employ his chief ſtudy. That no help may be wanting, all muſt be illuſtrated by ſuitable experiments, by the exhibition of ſpecimens, and the management of chemical proceſſes.

Nice and abſtruſe philoſophical opinions would not intereſt ſuch hearers; and *any* doctrines, inculcated in a refined manner, and referring to elaborate diſquiſitions of others, would not be underſtood by the major part of an audience of young perſons, as yet only beginning their ſtudies.

To this reſolution Dr. Black rigidly adhered, endeavouring every year to make his courſes more plain and familiar, and illuſtrating them by a greater variety of examples in the way of experiment. No man could perform theſe more neatly and ſucceſsfully. They were always ingeniouſly and judiciouſly contrived, clearly eſtabliſhing the point in view, and never more than ſufficed for this purpoſe. While he ſcorned the quackery of a ſhowman, the ſimplicity, neatneſs, and elegance, with which they were performed, were truly admirable. Indeed, the *ſimplex munditiis* ſtamped every thing that he did. I think it was the unperceived operation of this impreſſion that made Dr. Black's lectures ſuch a treat to all his ſcholars. They were not only inſtructed, but (they knew not how) delighted; and without any effort to pleaſe, but ſolely by the natural emanation of a gentle and elegant mind, cooperating, indeed, with a moſt perſpicuous exhibition of his ſentiments, Dr. Black became a favourite lecturer; and many were induced, by the report of his ſtudents, to attend his courſes, without having any particular reliſh for chemical knowledge, but merely in order to be pleaſed. This, however, contributed greatly to the extending the knowledge of chemiſtry; and it became a faſhionable part of the accompliſhment of a gentleman.

In the mean time, the path which had been opened by Dr. Black

to a new province of chemical reſearch began to be frequented by men of ſcience in various parts of Europe. It was not only a moſt unexpected and curious thing to find that a matter ſo ſolid and impenetrable as marble could appear in the form of air, and this air be again put into our hands in the form of marble; but this new acquaintance had properties which forcibly called for the moſt ſerious attention. This air can be poured from one jar into another, like as much water; and when it is poured out on a candle, or even on a fire, in ſufficient quantity, they are extinguiſhed in an inſtant, as if water had been poured on them. But further, ſhould a man take one full inſpiration of this air, this inſpiration will be his laſt; he expires without a groan. In ſhort, this is the deadly vapour which has often produced fatal effects in our mines. Being much heavier than common air, it glides downwards, collects in the drifts and lower parts of the mine, and ſometimes is ſo copious as to fill them, and even to riſe to a conſiderable height in the ſhaft. The unfortunate miner, let down by a rope to his work, as ſoon as his head gets under the ſurface of this fluid, drops off without any warning, and is daſhed to pieces, or ſuffocated by this choke-damp. But we have now learned its property of extinguiſhing flame; and it is uſual, before the workmen go down, to let down a choffer of burning coals, which are extinguiſhed as ſoon as they enter the fixed air. I may add, that it is this fluid that has long drawn the curious traveller to the Grotto del Cane in Italy, ſo called, becauſe a dog falls down dead as ſoon as he reaches the middle of the grotto.

On the other hand, it has been diſcovered that this very air, ſo

fatal, when applied to the nerves of the breathing organs, is moſt ſalutary, when received into the ſtomach; and, as an external application, it is moſt powerful in healing ulcerated wounds, cleanſing foul ſores, and in general, counteracting all tendency to gangrene or putrefaction in the diſorganiſed parts of the animal frame. Water impregnated with this air alleviates one of the moſt excruciating of human ſufferings, the pain of the ſtone or gravel.

It has alſo been diſcovered that this air, ſo deſtructive and ſo ſalutary, is forming in vaſt quantities every moment around us. Dr. Black diſcovered that the breathing of animals changes common air into fixed air; and that this change is accompanied by the emerſion of heat; which emerſion ſeems to be the principal ſource of the heat generated in the bodies of all breathing animals. Another moſt copious ſource of fixed air is the combuſtion of nine-tenths of all inflammable bodies. Every thing, which, in the courſe of burning, ſuffers the change which we call charring, changes common air into fixed air by burning in it. In this way does it ſometimes happen that perſons have been found lifeleſs, who have been ſhut up in a cloſe room with a charcoal fire. Laſtly, another abundant ſource of fixed air is the working of fermenting liquors, as they are ripening into wine or other intoxicating beverages. The froth and foam continually riſing from ſuch liquids is one of the pureſt kinds of fixed air.

Surrounded, then, as we are by ſources which are continually pouring in upon us this powerful ſubſtance, it behoves us to be on our guard; and we are highly obliged to him who gave us the means of detection, a method of removing it, and methods for procuring it,

when we would avail ourſelves of its ſalutary powers. Somewhat of all this had been known before 1756. The ingenious reſearches and experiments of Dr. Hales had occaſionally called our attention to ſome of the modes of the production of fixed air; but they appeared as ſingularities, inſulated facts, not connected in the general economy of nature, or, at leaſt, the connection was not obſerved. But now, having learned a ſure and eaſy teſt of the preſence of this kind of air, and underſtanding ſome of the means by which nature holds it accumulated in ſuch vaſt abundance, and ſome of the methods for ſetting it at liberty, ſo as to obtain it by itſelf, in circumſtances which lay it open to our examination, the chemiſts were enabled to detect it in almoſt every body that could be preſented to them, and were buſy in ſcrutinizing every ſubſtance with this view. They were particularly curious to examine every elaſtic eruption that they obſerved, on account of its reſemblance to thoſe eruptions in which fixed air is extricated. The efferveſcence of metals during their ſolution in acids reſembles ſo much the efferveſcence of acids and alkalis,—the frothy ebullition of ſome putreſcent mixtures reſembles ſo much the fermentation of wines and worts, that curioſity led immediately to the examination of the elaſtic matter which was extricated on thoſe occaſions. Theſe airs, however, were found altogether different from fixed air; but this circumſtance only fired the curioſity of the inquirers ſo much the more, and incited them to multiply experiments, and examine every body, in order to extricate or to create ſome elaſtic matter for a new ſubject of experiment.

Thus aroſe a new ſpecies of chemiſtry, chiefly converſant with

aëreal fluids, having an apparatus and a manner of proceeding altogether peculiar to itſelf, and ſo unlike all that we are hitherto acquainted with, that every thing may be ſaid to be big with curioſity and with novelty. This department of chemical ſcience got the name of PNEUMATIC CHEMISTRY.

Of all thoſe who were occupied in theſe reſearches, the moſt eminent were Dr. Prieſtley and Dr. Scheele. Dr. Prieſtley, by the number and variety of his experiments, and the ſubſtances he diſcovered; and Dr. Scheele, by the ingenuity, and the unwearied patience with which he examined the individual novelty which engaged his attention, and the ſagacity with which he contrived his experiments, ſo as to lead him with certainty to ſome important reſult, to be added to our former ſtock of chemical knowledge. Theſe two philoſophers, unknown to each other, diſcovered the ſame ſubſtances; ſubſtances which were acting the moſt important parts in the great operations of nature. Both of them diſcovered that aëreal fluid which alone ſuſtains the life of breathing animals, and which alone ſupports inflammation and combuſtion; for which reaſons it has been called *vital air* and *empyreal air*. Both of them alſo diſcovered another ſpecies of air, much more abundant, and indeed proved by Scheele to form the chief portion of our atmoſphere. He called it *foul* or *putrid air*, having diſcovered it firſt in the putreſcent fermentations. Dr. Prieſtley procured it from much leſs offenſive materials, and called it *phlogiſticated air*. It extinguiſhes life and fire as certainly as the fixed air of Dr. Black does, but is quite a different ſubſtance. Scheele firſt demonſtrated that our atmoſphere is a mixture,

but not a compound, of this and of vital air. Dr. Prieſtley alſo diſcovered ſome of the means by which nature removes all the taints which are occaſioned in the atmoſphere by the breathing of animals, the burning of fuels, and the fermentation of bodies, ſhewing that theſe corruptions are combinations of vital air with other elaſtic matters, and that all of them are again decompounded in the proceſs of vegetation, ſo that the plants reſtore to us the vital air in its original purity; thus accompliſhing one of the grand and beneficent circles of natural operations.

Such diſcoveries neceſſarily gave a dignity to pneumatic chemiſtry, which ſets it very high in the rank of natural ſciences. It is no longer confined to the ſtudy of thoſe properties of bodies which make them the ſubjects of human art, by which they are worked up for our purpoſes. We are now admitted into the laboratory of nature herſelf, and inſtructed in ſome of thoſe great proceſſes by which the author of this fair world makes it a habitable place, and a never-failing ſource of life and enjoyment, by a circle of beneficent changes, in which the ſame materials are made the means of enjoyment to ſucceſſive races of inhabitants, and are again and again preſented in their original purity and uſefulneſs.

So captivating to every mind of ſenſibility, it was no wonder that pneumatic chemiſtry became a very general ſtudy, and engaged the eager attention of the moſt accompliſhed in acquirements, and the moſt eminent in the ranks of ſociety. In Germany, in Italy, in Britain, in France, it found cultivators in every claſs of ſociety. The aëreal fluids having now become as familiar, and as eaſily managed,

as the tangible ſubſtances which we are accuſtomed to hammer, to grind, to diſſolve, and diſtil, they were mixed and ſubjected to all the torturing degrees of heat, and paſſed from retort to retort, and, in ſhort, were examined in every way that imagination could ſuggeſt. Such a pertinacious ſcrutiny of nature could not fail of bringing many things to light. The Honourable Mr. Cavendiſh has diſcovered that aquafortis conſiſts of two kinds of air; of vital air, the ſupport of life and of fire, and mephitic air, which extinguiſhes both. And he diſcovered that water, which, ſince the firſt dawn of natural philoſophy, has been conſidered as an element, is alſo compoſed of vital air, and of that air which ſometimes takes fire in our coal pits, and lays all waſte by its exploſion. Others have diſcovered that ſalt of hartſhorn conſiſts of inflammable and mephitic air; nay, that fixed air itſelf conſiſts of diamond diſſolved in vital air.

In the midſt of this ardour of reſearch, and this rich harveſt of diſcovery, Mr. Lavoiſier appeared, and took an active ſhare;—not hunting after new ſubſtances, he conſidered thoſe already known, with more ſagacity than the multitude buſy in the chace. He thought that the chemical relations of various ſubſtances had been miſtaken by all; that we hold many bodies as ſimple, of which we can ſhew the compoſition, and thoſe to be compounded which are really more ſimple. Thus, ſulphur, which the chemiſts, ever ſince the days of Stahl, have ſuppoſed to conſiſt of vitriolic acid, and that matter which imparts inflammability to bodies, was proved by Lavoiſier to be more ſimple than the acid, and that this acid was in fact compoſed of ſulphur and vital air. He proved that in the phenomenon which we call combuſ-

tion or inflammation, the only thing of which we are abſolutely certain is the combination of the inflammable body with vital air; and that, by ſeparating this air from it again, the body regains its primitive form, and is again inflammable, that is, again capable of uniting, in a particular way, with vital air. Reflecting now on the two caſes in which Dr. Black had diſcovered a combination of tangible matter with fire, in ſuch a way as not to be diſcovered by the temperature, but only by the liquid or the vaporous form which it cauſes the ſubſtance to aſſume, he aſſerted the reality of a third combination of tangible matter with heat, to be added to thoſe diſcovered by Dr. Black: A combination, which was not to be changed by the mere contact of a ſufficient quantity of *any* cold matter, but required the contact of another ſubſtance, properly related to vital air in the way of chemical affinity. Heat combined in this manner renders a fluid *aëreal*, or permanently elaſtic, and no longer condenſible like watery vapours. Theſe compounds he denominated *gaſes*. Of this kind are all the airs lately diſcovered. Laſtly, as the chief point of his doctrines, he affirms that the light and heat wich appear in the combuſtion, are ingredients of the vital air, detached from it, and from it alone, when its ponderable part combines with the body that we call inflammable.

The doctrine is not altogether new. Dr. Robert Hooke, one of the firſt members, and the brighteſt ornaments of the Royal Society of London, publiſhed the ſame doctrine in his *Micrography*, 1665; and Dr. Mayhow of Oxford entertained opinions extremely ſimilar; —Rey, a French chemiſt of that time, had ſimilar conjectures. But by ſome unaccountable fatality, theſe publications were forgotten.

This doctrine concerning combuſtion, the great, the characteriſtic phenomenon of chemical nature, has at laſt received almoſt univerſal adoption, though not till after conſiderable heſitation and oppoſition; and it has made a complete revolution in chemical ſcience.

But it is thought that the cultivation of pneumatic chemiſtry has given us even more important information; and it promiſes acquiſitions of knowledge of a ſtill more elevated rank. It promiſes admiſſion to the more myſterious operations of nature in the functions of vegetable and animal life. For, by the pneumatic analyſis, to which organized bodies have been ſubjected by ſome of the eminent chemiſts of the preſent day, it appears that they conſiſt of a very ſmall number of ſimple ſubſtances, capable of the aëreal form, but exiſting in the plant or animal in a liquid or a concrete ſtate, in conſequence of being differently combined or aſſociated. The laws of many of thoſe affinities have been diſcovered; and it is thought that we perceive, in many caſes, how, by a change of temperature, or by preſenting another ingredient, theſe combinations change, new forms are aſſumed, and the diſtinguiſhing products of animal and vegetable bodies appear. It is thought that we perceive, in ſome caſes, how the functions of the organized body produce this variation of temperature, or this change of ſituation among the ingredients, which occaſions the chemical combination obſerved.

On the other hand, we have now got ſome very diſtinct indications of the internal procedure of nature in thoſe ſpontaneous fermentations which take place in the contents of animal and vegetable bodies,

independent of their vital functions, and which at laſt deſtroy them, or reduce them to brute unorganized matter. Theſe changes are thought to proceed from ſmall variations of temperature or poſition, which, by changing the mutual forces of attraction, deſtroy that equilibrium of force which maintains things in their preſent condition. The particles change their partners, (ſo to expreſs myſelf) and in their new combinations, preſent to our view ſubſtances which were not exiſting before in the animal or vegetable matter. Thus does the ſweet juice of the grape give us wine, which did not exiſt before,—and this gives us inflammable ſpirits, which did not exiſt in the wine, or gives us ſour, mawkiſh, or fœtid liquors or fumes, none of which had any previous exiſtence.

Juſtly, therefore, have I ſaid that chemiſtry has riſen to a high and unexpected rank in the ſcale of ſcience, important to ſociety, by the vaſt additions which it has made to the power of man, and precious to the philoſopher, by the connections which it has brought into view between the different agents in the grand circle of natural operations which conſtitute growth, life, decay, and final deſtruction. I think myſelf equally entitled to ſay, that it was the two diſcoveries of Dr. Black,—fixed air, and combined heat, which gave the incitement, pointed out the road, and furniſhed the chief helps for purſuing it.

It is reaſonable to ſuppoſe that he took an active part in thoſe keen reſearches, which have thus occupied the attention of the philoſophers. But alas!—" his lot forbade." His conſtitution had always been exceedingly delicate. The ſlighteſt cold, the moſt trifling approach to repletion, immediately affected his breaſt, occaſioned fe-

veriſhneſs, and, if continued for two or three days, brought on a ſpitting of blood. In this ſituation, nothing reſtored him to eaſe but relaxation of thought and gentle exerciſe. The ſedentary life to which ſtudy confined him was manifeſtly hurtful; and he never allowed himſelf to indulge in any intenſe thinking, or puzzling reſearch, without finding theſe complaints ſenſibly increaſed.

Thus ſituated, Dr. Black was obliged to be contented as the ſpectator of the ſucceſsful labours of others. So completely trammeled was he in this reſpect, that although his friends ſaw others diſingenuous enough to avail themſelves of the novelties announced by Dr. Black in his lectures, without acknowledging the obligation, and were thence afraid that their friend's claim of originality and priority might become doubtful; and although they repeatedly urged him to publiſh an account of what he had done, this remained unaccompliſhed to the laſt. Dr. Black often began the taſk; but was ſo nice in his notions of the manner in which it ſhould be executed, that the pains he took in forming a plan of the work never failed to affect his health, and oblige him to deſiſt. Of this I ſaw a moſt diſtinct inſtance, when his diſſatisfaction with the artful conduct of Mr. Lavoiſier provoked him to make an unuſual exertion *.

* I embrace this earlieſt opportunity that occurs to correct a miſtake which I have made in page 220 of vol II, where I ſay, " Nor is he named in thoſe paſſages of the " earlier diſſertations where the character and properties of fixed air, and of mild and " cauſtic alkalis, are treated of." The words " *of the earlier diſſertations*," ſhould not have been there. The obſervation relates only to the joint memoirs of Lavoiſier and La Place, publiſhed by the Royal Academy of Sciences. Mr. Lavoiſier, in his firſt publication of *Opuſcles Phyſiques et Chymiques*, in 1774, gives a brief account of all that had been publiſhed or taught concerning quicklime, and fixed air, and effervescence. And

Dr. Black, therefore, devoted his whole time and attention to the communications which his pupils had a right to expect from him. Moderate in all his wiſhes, he was never anxious to bring himſelf into view, unleſs the occaſion required his appearance. His reputation naturally engaged him in an extenſive correſpondence, he being often appealed to as a judge, and often conſulted as a philoſopher. On ſuch occaſions, when he could give his opinion without being obtruſive, (a thing which he deteſted) he was ever ready to communicate it, and to give every uſeful information,—which he did with frankneſs and ſincerity, and with the moſt unaffected modeſty.

As to the manner in which Dr. Black acquitted himſelf in his public character of a profeſſor, I need only ſay that none contributed more largely to eſtabliſh, and ſupport, and increaſe, the high character which the Univerſity of Edinburgh has acquired. His talent for communicating knowledge was not leſs eminent than for obſervation and inference from what he ſaw. He ſoon became one of the principal ornaments of the Univerſity; and his lectures were attended by an audience which continued increaſing from year to year, for more than thirty years. It could not be otherwiſe. His perſonal appearance and manner were thoſe of a gentleman, and peculiarly pleaſing. His voice in lecturing was low, but fine; and his articulation ſo diſtinct

Dr. Black's performance has its ſhare of his attention, and his final approbation. But I muſt ſay, that it is with none of thoſe expreſſions of eſteem and reſpect which Mr. Lavoiſier profeſſes to have always entertained for the author. Dr. Black, at the time that he was offended with Mr. Lavoiſier's inſincerity, knew perfectly that he had taken notice of his doctrines concerning quicklime &c. in that early diſſertation: But the ground of offence was recent, and groſs.

that he was perfectly well heard by an audience conſiſting of ſeveral hundreds. His diſcourſe was ſo plain and perſpicuous, his illuſtration by experiment ſo appoſite, that his ſentiments on any ſubject never could be miſtaken, even by the moſt illiterate; and his inſtructions were ſo clear of all hypotheſis or conjecture, that the hearer reſted on his concluſions with a confidence ſcarcely exceeded in matters of his own experience.

I have already obſerved, that the ſtrong ſenſe which Dr. Black entertained of the duties of his profeſſional ſituation, precluded all extenſive medical practice, which otherwiſe he had every talent, and every accompliſhment fitted to inſure. He reſtricted his attendance as a phyſician to a few families of intimate and reſpected friends. He was, however, "a phyſician of good repute, in a place where the character "of a phyſician implies no common degree of liberality, propriety "and dignity of manners, as well as of learning and ſkill."

Averſe, by diſpoſition, from oſtentation, or any inclination to obtrude his opinions on the public, Dr. Black peculiarly diſliked appearing as an author. His diſſertation, *De Acido a cibis orto, et de Magneſiâ*, was a work of duty, being his Inaugural Theſis. His *Experiments on Magneſia, Quicklime, and other alkaline ſubſtances*, printed ſoon after, was almoſt indiſpenſably neceſſary for putting on a proper foundation what was only indicated in his inaugural diſſertation. His *Obſervations on the more ready freezing of water that has been boiled*, publiſhed in the Philoſophical Tranſactions of London in 1774, was alſo called for; and his *Analyſis of the waters of ſome boiling ſprings in Iceland*, made at the requeſt of his friend T. J. Stanley, Eſq. was read

to the Royal Society of Edinburgh, and publiſhed by the Council. Dr. Black was perhaps faſtidiouſly nice in his notions of a philoſophical performance, and too ſevere in his obſervations on the hurried publications of ſome chemiſts, which he uſed to call ſlovenly, and to conſider as literary manufacture for profit. But ſurely, every man who gives the public a new and important fact, confers a public benefit; and when he adds his own reflections and opinions, he only ſhews what have been his own incitements to exertion. Few perſons are ſo inſenſible to ordinary propriety, as to make pretenſions to authority; and if they did, it would be diſregarded, while the philoſopher would avail himſelf of the information communicated. Had Dr. Prieſtley and Dr. Scheele been as faſtidious as Dr. Black, we might at this day have been ſtill ignorant of the chief doctrines of chemical philoſophy. But ſuch was Dr. Black's averſion to all hypotheſis and conjecture in any experimental ſcience, that he could not endure the title of a *ſyſtem* to be given to any body of chemical doctrines yet publiſhed; and he did not call his own diſcourſes *Lectures on Chemiſtry*, but *Lectures on the Effects of Heat and Mixture;* ſo far did he think all his endeavours were from forming a ſyſtem of the chemical department of ſcience. In the laſt years of his life, he was convinced of the propriety of this ſcrupulouſneſs, by the precipitancy with which he ſaw young men, who had ſcarcely left the forms of the ſchool, publiſhing in all quarters of Europe. Intoxicated, as it were, with the large draughts of information afforded by pneumatic chemiſtry, they think themſelves adequate to the taſk of giving a ſyſtem of this almoſt boundleſs ſcience,—a ſyſtem which ſhall not leave a phenomenon unexplained;

and they obtrude theſe their crude conceptions on the public with moſt unbecoming confidence and authority. He ſaw the public pleaſed with this manner of proceeding, and far from being ſcrupulous about the ſolidity of the foundation, provided the ſtructure be ſhapely and extenſive. He dreaded the conſequences of this paſſion for theories. He therefore reſolved to abide ſcrupulouſly by his firſt plan, which he had adopted in the hour of calm reflection, and had modelled as much as he could on the rules of philoſophiſing, ſo warmly inculcated, and ſo ſcrupulouſly followed by the illuſtrious Newton. Scheele, Bergmann, and Berthollet, were the chemiſts of later times whom he thought moſt highly of, as Margraaf and Cramer were moſt admired by him among thoſe of older date. He correſponded occaſionally with Seguin, and with Crell, who had been his pupil; but did not encourage much intercourſe of this kind, having found that his informations ſometimes appeared in print as the inveſtigations of the publiſhers. He could not be engaged to tranſmit any eſſay to the Royal Academy of Sciences at Paris, or the Imperial Academy at St. Peterſburgh, of both of which he had been elected a foreign aſſociate.

Such was Dr. Black, conſidered as a public man. I wiſh that I could as eaſily deſcribe him in his private capacity,—at home, or in ſociety,—as an acquaintance, or a friend: But this requires a talent to which I have no pretenſions. It is not a very difficult matter to draw a figure, which ſhall ſhew with abundant accuracy, any peculiarity of a man's dreſs, and perhaps even give ſomewhat of his air; but the delicate ſtrokes which mark his features, and ſhall make us know the man, require the hand of a painter.

I have already obſerved, that when I was firſt acquainted with Dr. Black, his aſpect was comely and intereſting. As he advanced in years, his countenance continued to preſerve that pleaſing expreſſion of inward ſatisfaction, which, by giving eaſe to the beholder, never fails to pleaſe. His manner was perfectly eaſy, and unaffected, and graceful. He was of moſt eaſy approach, affable, and readily entered into converſation, whether ſerious or trivial. His mind being abundantly furniſhed with matter, his converſation was at all times pertinent and agreeable: For Dr. Black's acquirements were not merely thoſe of a man of ſcience. He was a ſtranger to none of the elegant accompliſhments of life. He therefore eaſily fell into any topic of converſation, and ſupported his part in it reſpectably. He had a fine or accurate muſical ear, and a voice which would obey it in the moſt perfect manner; for he ſung, and performed on the flute, with great taſte and feeling; and could ſing a plain air at ſight, which many inſtrumental performers cannot do. But this was ſcience. Dr. Black was a very intelligent judge of muſical compoſition; and I never heard any perſon expreſs ſo intelligibly the characteriſtic differences of ſome of the national muſics of Europe. I ſpeak of Dr. Black as I knew him at Glaſgow: After his coming to Edinburgh, he gave up moſt of thoſe amuſements. Without having ſtudied drawing, he had acquired a conſiderable power of expreſſion with his pencil, both in figures and in landſcape. He was peculiarly happy in expreſſing the paſſions; and ſeemed, in this reſpect, to have the talent of a hiſtory painter. He had not had any opportunities of becoming a connoiſſeur; but his opinion of a piece of painting, or ſculpture, was reſpect-

ed by good judges. Figure, indeed, of every kind, attracted his attention;—in architecture, furniture, ornament of every ſort, it was never a matter of indifference. Even a retort, or a crucible, was to his eye an example of beauty or deformity. His memorandum books are full of ſtudies (may I call them) of this ſort; and there is one drawing of an iron-furnace, fitted up with rough unhewn timber, that is finiſhed with great beauty, and would not diſgrace the hand of a Woollet. Naturally, therefore, the young ladies were proud of Dr. Black's approbation of their taſte in matters of ornament. Theſe are not indifferent things; they are features of an elegant mind, and they account for ſome part of that ſatisfaction and pleaſure which perſons of all different habits and purſuits felt in Dr. Black's company and converſation.

I think that I could frequently diſcover what was the circumſtance of form, &c. in which Dr. Black perceived or ſought for beauty,—it was ſome ſuitableneſs or propriety; and he has often pointed it out to me, in things where I never ſhould have looked for it. Yet I ſaw that he was ingeniouſly in the right. I may almoſt ſay that the love of propriety was the leading ſentiment of Dr. Black's mind. This was the firſt ſtandard to which he appealed in all his judgments; and I believe he endeavoured to make it the directing principle of his conduct. Happy is the man whoſe moderation of purſuits leaves this ſentiment in poſſeſſion of much authority. Seldom are our judgments greatly wrong on this queſtion; but we too ſeldom liſten to them.

Dr. Black had the ſtrongeſt claim to the appellation of a man of propriety and correctneſs. His friend Dr. Ferguſon knew him well,

and can delineate his moral features infinitely better than I can. Dr. Ferguſon ſays of him,—

" As Dr. Black had never any thing for oſtentation, he was, at all " times, preciſely what the occaſion required, and no more. Much " as he was engaged in the details of his public ſtation, and chemical " exhibitions, his chambers were never ſeen lumbered with books " and papers, or ſpecimens of mineralogy, &c. or the apparatus of " experiments. Nor did any one ſee Dr. Black hurried at one time " to recover matter which had been improperly neglected on a for- " mer occaſion. Every thing being done in its proper ſeaſon and " place, he ever ſeemed to have leiſure in ſtore; and he was ready " to receive his friend or acquaintance, and to take his part with " cheerfulneſs in any converſation that occurred. And, let me re- " mark, that no one ever with more eaſe to himſelf refrained from " profeſſional diſcuſſions of any ſort, or converſation in which he " was acknowledged ſuperior,—or with leſs ſelf-denial, in mixed com- " pany, left the ſubject of converſation to be choſen by others." Yet was he far from maintaining a ſilence indicating either indifference or neglect; on the contrary, he loved to promote ſocial converſation by every cheerful thought that occurred. " Many years " member of a ſociety of noblemen and gentlemen of the firſt rank, " of judges, lawyers, military men, and profeſſed men of letters, he " kept his place with the moſt eaſy propriety, having knowledge ſuf- " ficient for giving him an intereſt in the converſation of each, and " for taking a reſpectable ſhare in it, without exhibiting any pecu- " liarities ariſing from his more accuſtomed habits of thought."

This ſociety, and another ſmall evening party, or club of gentlemen, more profeſſedly ſcholars, were the only public companies which his delicate health permitted him to frequent.

But Dr. Black had neither the temper nor the feelings of a recluſe. He loved ſociety, and felt himſelf beloved in it. In coming to Edinburgh, he had the happineſs of rejoining his friend, Mr. Adam Smith, whoſe ſociety and friendſhip had given him ſo much delight in Glaſgow. The ſame prominent feature of character, "perfect ſingleneſs of heart," void of all guile, attached him warmly to Mr. David Hume; and the attachment was mutual and equally confidential. His relation Dr. Adam Ferguſon, Mr. John Home, Dr. Alexander Carlyle, and one or two other gentlemen of talents and of elegant accompliſhments, were his chief ſociety in his hours of relaxation. His profeſſional eminence was not the bond of this acquaintance; for their attachment to him, and to each other, aroſe from their experience of ingenuity, and candour, and good taſte, rather than from any ſimilarity of ſtudies, or agreement of opinions.

But there were others of Dr. Black's intimate friends, to whom his philoſophical talents, and more particularly his chemical and geological knowledge, were powerful ſources of attachment. Mr. Clerk of Elden, and his brother Sir George, Dr. Roebuck, and Dr. James Hutton, were ever as ready to receive information from him as he could be to impart it; and in their ſociety he could indulge in his profeſſional ſtudies. To the laſt of theſe gentlemen Dr. Black was moſt affectionately attached; and in reſpect to habits of intimacy,

Dr. Hutton ſhould perhaps have been placed firſt on this liſt. " He " made up in phyſical ſpeculation all that was wanting in any of the " reſt of his acquaintance. Yet would it be difficult to ſay whether " the characters of Dr. Black and Dr. Hutton, ſo often ſeen toge- " ther, were moſt to be remarked for reſemblance or contraſt. Both " profound in phyſical ſcience; both rigid adherents to fact, in exclu- " ſion of all hypotheſis, or the moſt ſpecious conjecture: Both of " conſummate humanity and candour. Dr. Black was ſerious, but " not moroſe. Dr. Hutton playful, without petulance. The one " was always on ſolid ground; and of him it might be ſaid, *nil moli-* " *tur ineptè.* The other, whether for pleaſantry, or ſerious reflec- " tion, could be in the air, ſpeculate beyond the laws of nature, or " their phenomena, and treat the common notion of body, extended " and impenetrable, as a vulgar error." But, with all this diverſity of reliſh, the friends were united by mutual reſpect for the talents of each other, and the moſt implicit confidence in each other's integrity and worth. Dr. Hutton was the only perſon now near him, to whom Dr. Black imparted every ſpeculation in chemical ſcience, and who knew all his literary labours. Seldom were the friends aſunder for two days together. Mr. Watt was now at a diſtance, in Birmingham; but they kept up a cloſe and philoſophical correſpondence, in which all their ſpeculations and projects were known to each other. Soon after his coming to Edinburgh, the Doctor got another pupil, Mr. Archibald Geddes, manager of the glaſs-works at Leith, who ſoon engaged his Profeſſor's attention by the readineſs and propriety with which he applied to the improvement of his manufac-

ture the inſtructions which he received in the lecture. Farther acquaintance ſhewed more to eſteem and attach; and it terminated in the moſt intimate and confidential friendſhip. From this friend no circumſtance of Dr. Black's former life or his preſent condition was withheld; and to his aſſiſtance he had recourſe in every thing that affected either his fortune or his comfort.

In this ſociety Dr. Black paſſed his days calmly, but cheerfully, reſpected and beloved, and conſcious that he was worthy of this regard. Though unmarried, his houſe was not unoccupied; for the uniformity of his ſingle life was often agreeably checquered by the welcome viſits of his numerous friends, the deſcendants of the reſpectable pair at Bordeaux. In theſe domeſtic ſcenes he appeared to the beſt advantage, for he enjoyed them moſt. Large companies were not to his taſte. Parties of men of learning, he thought, had ſeldom much of conviviality; this requires converſation of more general intereſt, and where the affections are rather engaged than the underſtanding.

My narration now draws toward a cloſe. The infirmities of advanced life now bearing more heavily on a feeble conſtitution, gradually curtailed thoſe hours of walking and gentle exerciſe which had always been neceſſary for Dr. Black's eaſe. Company and converſation began to fatigue; he went leſs abroad, and was viſited only by his intimate friends. His duty at College now became too heavy a taſk, and he got an aſſiſtant, who took a ſhare of the lectures, and relieved him from the fatigue of the experiments. But, at length, even this was more than enough for his diminiſhed ſtrength, and he was obliged to give over lecturing altogether. But all this gradual de-

cline, and occaſional ſhort periods of more than common indiſpoſition, made no change in the gentle cheerfulneſs of his diſpoſition and manners. A friend was always received with unaffected welcome; and whenever he found himſelf engaging more in converſation than was conſiſtent with his health, he ſaid ſo; and ſaid it in a manner ſo pleaſing, that his gueſt was only the more induced to repeat his viſit.

I cannot conclude what ſhould be ſaid of this amiable perſon ſo well as in the words of Dr. Ferguſon, who ſays,

" The life of Dr. Black was not leſs diſtinguiſhed by correctneſs " and propriety of conduct, than by ingenious reaſoning and ſcienti- " fic reſearch. For he carried into his private affairs the ſame order " and good conduct that he employed in his philoſophical ſtudies, and " in his profeſſional duties. And he reaped through life the bene- " fit of his attention to this particular, in the eaſe of his circumſtances, " and in the power which it gave him on occaſion to aſſiſt his friends, " or to contribute to the promoting of any public convenience.

" From thoſe, indeed, with whom mere remiſſneſs or neglect is al- " lowed to paſs for generoſity, Dr. Black may have been thought too " attentive to the increaſe of his fortune. But they did not know " him*; and if they were required to ſubſtantiate the charge, the proofs " which they would adduce would be found indiſtinguiſhable from " the real effects of ſound reaſon and good ſenſe. His expences were " indeed regulated, but in no way unbecoming his ſtation. His houſe

* About the time that he left Glaſgow, he loſt three-fourths of all the fruits of his labours, by the failure of the houſe where he had lodged his money. He foreſaw this failure for two years; yet no man ever obſerved the ſmalleſt appearance of fretfulneſs, or any alteration of his behaviour to the perſon by whom he was to ſuffer ſo ſeverely.

" was ſpacious; and his table, at which he never improperly declined
" any company, was plentiful and elegant,—rather above than below
" his condition. His contributions for all public purpoſes were
" liberal, and like a gentleman; and his purſe was open to aſſiſt his
" friend *. Much of his practice as a phyſician aroſe from his pre-
" vious connection with the patient as a friend; and he was as aſſi-
" duous where he would not accept, or could not expect a fee, as in
" the moſt lucrative part of his profeſſion."

I have already ſaid that Dr. Black's conſtitution never was robuſt, and that, as he advanced in years, it became gradually more delicate and frail; ſo that every cold he caught occaſioned ſome degree of ſpitting of blood. Yet he ſeemed to have this unfortunate diſpoſition of body almoſt under command, ſo that he never allowed it to proceed far, or to occaſion any diſtreſſing illneſs; and he thus ſpun his thread of life to the laſt fibre; and even this does not ſeem to have broken, but merely to have ended. " He guarded againſt illneſs, by
" reſtricting himſelf to a moderate, or I ſhould rather call it, an ab-
" ſtemious diet; and he met his increaſing infirmities with a propor-
" tional increaſe of attention and care,—regulating his food and ex-
" erciſe by the meaſure of his ſtrength. It is wonderful with what
" ſkill and ſucceſs he thus made the moſt of a feeble conſtitution, by
" thus preventing the acceſs of diſeaſe from abroad. He enjoyed a
" health which was feeble indeed, but ſcarcely interrupted, and a mind

* I could give more than one or two inſtances in which a great part of his fortune was at riſk for his friend.

" ever undiſturbed, in the calm and cheerful uſe of all his faculties. " A life ſo prolonged had the advantage of preſent eaſe, and the " proſpect, when the juſt period ſhould arrive, of a calm diſſolution." —His only apprehenſion was that of a long continued ſick bed; and and this perhaps leſs from any ſelfiſh feeling, than from the humane conſideration of the trouble and diſtreſs occaſioned to attending friends; and never was this modeſt and generous wiſh more completely gratified. " On the 26th Nov. 1799, and in the ſeventy-firſt year " of his age, he expired, without any convulſion, ſhock, or ſtupor, to " announce or retard the approach of death. Being at table, with " his uſual fare, ſome bread, a few prunes, and a meaſured quantity " of milk, diluted with water, and having the cup in his hand when " the laſt ſtroke of his pulſe was to be given, he had ſet it down on " his knees, which were joined together, and kept it ſteady with " his hand, in the manner of a perſon perfectly at eaſe; and in " this attitude expired, without ſpilling a drop, and without a writhe " in his countenance; as if an experiment had been required to ſhew " to his friends the facility with which he departed." His ſervant opened the door to tell him that ſome one had left his name, but getting no anſwer, ſtepped about half way towards him, and ſeeing him ſitting in that eaſy poſture, ſupporting his baſon of milk with one hand, he thought that he had dropped aſleep, which he had ſometimes ſeen happen after his meals. He went back, and ſhut the door; but before he got down ſtairs, ſome anxiety, which he could not account for, made him return and look again at his maſter. Even then, he was ſatisfied, after coming pretty near him, and turned to go away;

but again returned, and coming quite close to him, he found him without life.

" So ended a life, which had passed in the most correct applica-
" tion of reason and good sense to all the objects of pursuit which pro-
" vidence had prescribed to his lot;" with many topics of agreeable recollection, and few things to ruffle his thoughts. He had long enjoyed the tender and affectionate regard of parents whom he loved, honoured, and revered; with the delightful consciousness of being a dutiful son, and being cherished as such;—one of a family remarkable for sweetness of disposition and manners, he had lived with his brothers and sisters in terms of mutual love and attachment. He had never lost a friend, but by the stroke of mortality, and he felt himself worthy of that constancy of regard. He had followed a profession altogether to his taste; and had followed it in a manner, and with a success, which procured him the esteem and respect of all competent judges, and set his name among the most eminent, and he was conscious that his reputation was not unmerited; and with a success, in respect of emolument, which secured the respect, even of the ignorant; which gave him the command of every rational gratification, and enabled him to add greatly to the comforts of the numerous descendents of his worthy parents,—heirs, not only of their name, but likewise of their unambitious moderation, and amiable simplicity of character. " The fortune that he left shewed how much he had
" profited by the order and just arrangement which he had ever main-
" tained in his affairs, amounting almost to the double of what any

" one thought that his income and his frugality could have amaſſed.
" The whole was diſpoſed of by will, in a moſt accurate and ſa-
" tisfactory manner, parcelled into ſhares, according to the degree in
" which each individual was the object of his care and ſolicitude."

His ſaltem accumulem donis, et fungar inani
Munere.———

LECTURES

ON

CHEMISTRY.

OF CHEMISTRY IN GENERAL.

CHEMISTRY, like all other ſciences, has ariſen from the reflections of ingenious men on the general facts which occur in the practice of the various arts of common life. It will not greatly conduce, gentlemen, to your progreſs in the ſcience, to trace chemiſtry from its firſt humble appearance in the practice of the more ingenious artiſts and manufacturers, till, by a copious collection and judicious arrangement and compariſon of thoſe practices, men ſtill more ingenious and ſpeculative deduced certain fixed laws of material nature, according to which all the chemical phenomena proceeded, and thus have raiſed this department of ſtudy to the rank and character of a ſcience, or ſyſtem of general doctrines, expreſſive of thoſe laws of nature, and ſo arranged, according to the principles of ſound logic, as to carry evidence and conviction into its various branches, and to furniſh maxims and precepts for ſucceſsfully directing the practice in a great variety of the uſeful arts. This, though not unuſual in the beginning of ſuch a courſe of lectures, cannot, I imagine, be attended with any conſider-

able advantage, becauſe, to perſons altogether unacquainted with the chemical facts, and even with the ſubſtance in the hands of the chemiſt, ſuch a narration would be little more than the mentioning of many names, and telling you ſomething of each, of which you could not form any diſtinct conception.

I hold it better, therefore, to turn your whole attention to the preſent ſtate of chemiſtry, and give you ſuch a deſcription of it as ſhall correſpond with our preſent attainments in the ſcience, fully comprehending them all, while it ſhall exclude every thing foreign from our proper ſtudy. I ſhall give you a deſcription or definition of it. This being ſomewhat peculiar, I muſt beg your indulgence, while I give my reaſons for being diſſatisfied with the definitions which have been given of chemiſtry by others; and I truſt that you will not think your time miſemployed in attending to them, becauſe, in ſtating my objections to the definitions given by others, I ſhall be led to point out the eſſential differences between chemiſtry and other branches of knowledge with which it has been confounded.

In the firſt place then, one manifeſt impropriety in the greater part of the definitions of chemiſtry is, that their authors repreſent chemiſtry as an art. This error is to be ſeen in the definitions given by authors of the higheſt name, for ſuch ſurely were Boerhaave and Stahl. Yet it is plain, from the writings of thoſe authors, that their idea of chemiſtry did not correſpond with ſuch a denomination. They were probably led into this miſtake by confining their attention too much to the chemical books which appeared before the beginning of this century. The greater number of thoſe books had, indeed, for their principal object, a chemical art—the art of pharmacy: and, although they bore the title of courſes of chemiſtry, they contained little more than the detail of proceſſes, or rules, whereby a great number and variety of ſubſtances were prepared for the uſes of medicine, with occaſional ſervices to other arts, particularly metallurgy. At the ſame time, alſo, thoſe commonly known by the name of chemiſts

were chiefly, or ſolely, artiſts, employed in making certain chemical preparations and products, according to rules and directions which they had learned to obſerve. But ſurely ſuch perſons, confining themſelves to the exerciſe of an art, and, perhaps, deſcribing and teaching this art in the moſt judicious and complete manner, are ſtill upon a footing with all other chemical artiſts, ſuch as brewers, diſtillers, dyers, and many others; and, if we chooſe to apply the term chemiſt to them, we muſt find ſome other term to diſtinguiſh from theſe artiſts ſuch men as Sir Iſaac Newton, Mr. Boyle, Cavendiſh, Prieſtley, Boerhaave, Scheele, Bergmann, Lavoiſier, and others, who have improved our ſcience. The public now perceive the diſtinction, and in ſome meaſure make it, by calling the chemical artiſts TRADING CHEMISTS.

From what I have now ſaid, you may perceive ſomething of the diſtinction which I think neceſſary to keep in view between art and ſcience, between the artiſt and the man of knowledge, or the philoſopher. The man of knowledge, the philoſopher, is he who ſtudies and acquires knowledge in order to improve his own mind; and with a deſire of extending the department of knowledge to which he turns his attention, or to render it uſeful to the world, by diſcoveries, or by inventions, which may be the foundations of new arts, or of improvements in thoſe already eſtabliſhed. Excited by one or more of theſe motives, the philoſopher employs himſelf in acquiring knowledge and in communicating it. The artiſt only executes and practiſes what the philoſopher or man of invention has diſcovered or contrived, while the buſineſs of the trader is to retail the productions of the artiſt, exchange ſome of them for others, and tranſport them to diſtant places for that purpoſe.

I may illuſtrate this diſtinction between the man of ſcience and the artiſt ſtill more by an example or two. While Sir Iſaac Newton was employed in his experimental inquiry concerning the nature and laws of light, and was led by his genius from one beautiful diſcovery to

another, until he produced the admirable work which he has left us on this ſubject, he was acting the part of the philoſopher. His obſervations and diſcoveries induced him to believe that, by reaſon of the nature of light, and the manner in which it is affected by tranſparent refracting mediums, the teleſcopes in uſe were capable of only a very limited degree of magnifying power. But the manner in which it was reflected exhibited no ſuch obſtacles to amplification. He therefore propoſed that teleſcopes ſhould be attempted, which ſhould perform their effect by reflection; by which he ſaw that a much more perfect and diſtinct image might be formed. He went ſtill farther: He tried different mixtures of metals, to learn which would form the beſt compoſition for the mirrors which muſt be employed in theſe teleſcopes inſtead of the glaſs lenſes hitherto employed. He even tried different ways of grinding and poliſhing thoſe mirrors, in order to learn how to give them the moſt perfect figure and the moſt exquiſite poliſh, ſo as to reflect the brighteſt and moſt perfect image. After having thus completed all the improvements he had projected, although he publiſhed directions for making a reflecting teleſcope in perfection, he was, in all this, ſtill acting the part of the philoſopher. The perſon who merely put in practice the directions given by Newton, following implicitly the rules which he had laid down, was the humble artiſt. Sir Iſaac Newton, for want of good workmen, acted this part alſo, in all its detail, being unable to obtain a good reflecting teleſcope in any other way than that of making it himſelf, according to his own rules.

In like manner, we find numerous operators who, either with their own hands, or by the hands of others whom they employ, exerciſe the various branches of the valuable art of pottery. Theſe perſons, by an apprenticeſhip, or otherwiſe, have learned how to chooſe and to mix the proper materials; how to form the veſſels; to apply the glazing and other decorations; and laſtly, how to give the proper degree of fire to conſolidate and finiſh the ware. Theſe are all artiſts, while

they only exert in practice the ſkill they have acquired, whether by communication from others, or by efforts of their own ingenuity. But if there be a Wedgewood among them, who takes pleaſure in attaining more extenſive knowledge of the ſubject, who, by comparing the practice of other potters with his own, and by making new trials, and varying the compoſition, the glazing, the firing, and other parts of the proceſs, endeavours to make improvements upon the art, or to underſtand it better than before; ſuch a perſon, in my opinion, is a philoſopher, or a man of thought, ſtudy, and invention. Even in medicine, the ſame diſtinction may, I think, be very properly made. The phyſician who only practiſes what he has learned, treats his patient as we ſay *ſecundum artem*, and gives himſelf no further trouble, ſhould be reckoned an artiſt: But, when he beſtows uncommon attention and ſtudy upon the diſeaſes he has occaſion to treat, endeavours to underſtand them better than ordinary, or to improve the method of curing them, he, in ſo far, is certainly one of the moſt uſeful philoſophers—a medical philoſopher *.

It may be objected, perhaps, that I uſe ſome freedom with common language in this manner of applying the term philoſopher, when I do not confine it, as is commonly done, to men of great learning and re-

* Does not the diſtinction between the philoſopher and the artiſt conſiſt purely in this: That the latter employs with ſucceſs certain proceſſes and manipulations with the ſubjects of his art, without being able to give any other reaſon for his proceeding but that he was ſo directed to act, and has always found theſe precepts completely effective? And, in communicating this art to another perſon, he merely inculcates certain precepts which, he tells him, will never fail; while the philoſopher perceives, by his knowledge of the laws of nature, how every operation is efficient, and muſt, in conformity to thoſe laws, be followed by the deſired effect, and by no other. The artiſt may, like Wedgewood, improve his art greatly, by dint of many trials, either made at random, or by reflecting on his former practice: But the philoſopher can improve the art, and even invent new arts, by proceeding from fixed principles, and applying them with propriety; and he ſees clearly the reaſon of every effect in the nature of the means employed. EDITOR.

tirement, but apply it to any man who endeavours to acquire knowledge, or thinks and reaſons upon any uſeful ſubject. In this ſenſe, the term, it may be ſaid, will apply to a plain farmer, if he only ſtudies the conſtruction of his plough, and how far it is adapted to produce in the beſt manner the effect for which it is intended, and perhaps endeavours to improve it. And, in ſo far as he does this, I have no ſcruple to reckon him a philoſopher; a ruſtic one, he may poſſibly be thought, but a more uſeful one than many who think the title indiſputably theirs. Men of great learning and retirement often contribute little or nothing to the progreſs of improvement. They ſpend their time in learning and admiring the inventions of others, without ever propoſing a new thought of their own, or ever diſcovering one uſeful power in nature.

If this diſtinction, therefore, between ſcience and art be allowed me, I would not define the ſyſtem of knowledge which I propoſe to comprehend in theſe lectures, by ſaying that it is an art. An art is a ſet of rules or directions for the uſe of an artiſt, who, by practiſing them, is enabled to obtain certain productions which are the object of the art. A ſcience is a body, or ſyſtem of knowledge; and chemiſtry, as now generally ſtudied and taught, is undoubtedly a ſcience, which, though it has given origin to numerous arts, is diſtinct from them all, as you will more eaſily perceive when we are farther advanced in this courſe.

But, ſetting aſide this impropriety in ſome definitions of chemiſtry, what has been added to diſtinguiſh it from other branches of knowledge is not leſs exceptionable. The object of it is commonly ſaid to be the reſolution, or diviſion, of compounded bodies into the principles of which they are compoſed, and the production of new compounds, by combining bodies which were before in a ſeparate ſtate. This will be found to be the meaning of Dr. Stahl's definition, though delivered in terms that are a little abſtruſe. And the ſame diſtinction

is expreſſed more clearly in Mr. Macquer's, which, as being one of the lateſt, may be taken as an example *.

But, when this manner of defining chemiſtry is better conſidered, it will be found improper upon ſeveral accounts;—as, firſt, In a great number of the experiments or operations which the chemiſt performs, he neither divides compounded bodies into their principles, nor combines others which were ſeparated before, nor has he either of theſe ends in his view in many of his inquiries. To form an idea of the ſort of knowledge which he poſſeſſes, and of the manner in which his mind is commonly employed, we need only to read the chemical hiſtory of any particular ſubſtance, as contained in the ordinary ſyſtematical books; or we may look to the account of ſome late chemical inquiry into the nature of a newly diſcovered foſſil, or other object, which had not been much examined before. If we obſerve what particulars have drawn the chemiſt's attention, and what points he has been anxious to inquire about, we find that he firſt examined its external appearance, in order to form ſome judgment of its nature, by comparing it with a number of other bodies he already knows. After examining its external appearance, he ſtraightway inquires whether or not it reſembles them in other reſpects. He puts it into the fire, to learn if it be inflammable, or whether its colour, texture, weight, or other qualities, can thus be changed; whether it melts or aſſumes the form of vapour, and at what particular degree of heat it undergoes theſe changes. He mixes it with a variety of other bodies, ſolid or fluid, in order to learn whether or not it diſſolves in the fluids, or any of them, and if it does, whether any changes of colour, odour, or other qualities, are thus induced. And many of theſe laſt trials are diverſified by the application of heat at the ſame time. Now, I aver that we ſhould give a very improper account of his employment and

* La Chymie eſt une ſcience dont l'objet eſt de connoitre la nature, et les proprietés des tous les corps, par leurs analyſes et leurs combinaiſons.—*Dict. de Chymie.*

object on this occaſion, if we define it by ſaying that it is only to compound the foſſil with other bodies, or to ſeparate the ingredients of which it is compoſed. When he examines the facility wherewith it is melted, or converted into vapour; the particular manner in which it burns, if it be an inflammable body; the changes of colour, texture, conſiſtency, odour, or other qualities, induced in it by the multiplicity of his experiments,—he certainly is not ſtudying to decompound the ſubject of his inquiry, or to combine it with other bodies. If it ſhould happen, during the courſe of his reſearch, that any appearances preſent themſelves which give him reaſon to think that it is a compounded body, ſeparable into ſubſtances of different kinds, he will inveſtigate its nature in this reſpect alſo, and will apply various means which he knows to be effectual in ſeparating the ingredients of other bodies. But although he ſhould thus find it to be a compounded ſubſtance, and be able to ſeparate the different ingredients of which it is compoſed, he will conſider this diſcovery as only a part of the chemical hiſtory of this new object, the nature of which he was thus inveſtigating.

Dr. Boerhaave appears to have perceived the impropriety of the definitions which had been attempted by others, and propoſed one, in which he gave a new diſtinction, or character, of chemiſtry. We find his idea of chemiſtry the moſt clearly expreſſed in ſome paſſages of his *Methodus Studii Medici* and of his *Elementa Chemiæ*. He aims at it too in the definition which he gives in the beginning of his elements; but that definition is expreſſed in ſuch general terms, that it cannot be underſtood by itſelf. In his *Methodus Studii Medici* we find his meaning fully expreſſed. Recommending the ſtudy of chemiſtry to the phyſician, he ſays that it is a principal, or rather the moſt important branch of the ſtudy of nature; and he characteriſes it by adding, that, whereas what is commonly called natural philoſophy is the ſtudy of the more general qualities and affections of matter, ſuch as external form, number, bulk, weight, motion, reſt, chemiſtry is the

inveſtigation and ſtudy of the more particular qualities, the qualities peculiar to each diſtinct kind of matter, many of which qualities are of the greateſt importance to mankind: ſuch as the polarity of the magnet, and its power to attract iron and ſteel; the diſpoſition of ſteel to receive the ſame powers from the touch of the magnet; the exploding quality of gunpowder; the ſoftneſs and flexibility of lead; the hardneſs and ſtrength of iron; the denſity and durability of gold; the poiſonous quality of arſenic; the nouriſhing quality of wheat and other grains; the ſweetneſs of ſugar and of honey; and innumerable others. Theſe are all qualities which are found in thoſe particular bodies, but not in bodies in general; and Dr. Boerhaave thought it to be the characteriſtic of chemiſtry that it was the ſcience which treated of theſe particular qualities of bodies *. I find myſelf, however, under the neceſſity of diſſenting from the Doctor upon this point, for the following reaſons.

If it be made the diſtinction of chemiſtry, that it is employed in conſidering the particular qualities of bodies, in oppoſition to thoſe general ones which belong equally to all kinds of matter, a great many things will be comprehended among the objects of this ſcience which in reality do not belong to it, but to other ſciences: and the bounds of it will therefore be extended in ſome reſpects a great deal too far. To give examples, we may begin with ſome of thoſe very qualities which Dr. Boerhaave has quoted as objects of chemiſtry, or of the chemiſt's attention. The attracting and polar qualities of the magnet and of ſteel, when rightly prepared. Theſe are qualities particularly belonging to theſe bodies; and, for this reaſon, Dr. Boerhaave has cited them as qualities, the ſtudy and knowledge of which employed the attention of the chemiſt more than of any other philoſopher, and which he could beſt explain. But it is certain that he has done this improperly. The chemiſts have not paid any particular attention to the polarity of

* See Method. Studii Medici, 1751, page 133. Elementa Chemiæ, Paris, 1733, page 45.

the magnet, or the nature of its attractive power. They avail themſelves of this power indeed when they wiſh to purify iron filings; and they take ſome notice of theſe qualities when they treat of that particular ore of iron called loadſtone, or magnet. But they have ſtudied the nature of this ſtone, not on account of theſe qualities, but as it is an ore of iron. The books, in which you will find the attracting qualities and polarity of the magnet, and of magnetical ſteel, moſt fully conſidered, are not the books of chemiſtry, but the books of experimental or mechanical philoſophy. The ſame may be ſaid of the electrical qualities of the tourmalin, the ſtudy of which belongs to the electrician; as alſo of the qualities of Iceland cryſtal with reſpect to light, to which the attention of the optician is directed, not that of the chemiſt. But beſides theſe, we have ſtill a long liſt of particular qualities to add, the ſtudy of which certainly does not belong to the chemiſt. Theſe are the virtues of all the remedies employed by phyſicians to act on the body; the purgative quality of rhubarb, the emetic one of ipecacuanha, and ſo on of all the reſt. Theſe are certainly particular qualities; but no perſon will ſay that it is the chemiſt who ſtudies them, or ought to ſtudy them the moſt. It is the phyſician who ought to be beſt acquainted with them; and the ſtudy of them has always been conſidered as a neceſſary branch of his ſcience. Dr. Boerhaave has been ſo little aware of this, that he has mentioned ſome of thoſe very qualities, by which different ſubſtances act on the human body, as objects of chemical ſtudy; ſuch as, the poiſonous quality of arſenic. "Sed chemia invenit per ſua experimenta vim de-" leteream ineſſe huic arſenico." But ſurely this is without foundation. I will be bold to ſay that there is no chemical experiment from which it could be inferred that arſenic is poiſonous.

Thus it appears that Dr. Boerhaave's definition takes in many things which do not belong to this ſcience. I will further aver, that it leaves out many things which do really belong to it. The various kinds

of matter have many reſemblances to one another in their diſpoſition to be affected by heat. They have *general qualities*, or a *general nature*, with regard to heat, which become objects of the chemiſt's attention and ſtudy, but which Dr. Boerhaave excludes by his definition. They have alſo a few *general qualities*, which they ſhow in their manner of mixing with one another, and which are undoubtedly objects of the chemiſt's attention, but which are alſo thus excluded by Dr. Boerhaave.

I find myſelf, therefore, under the neceſſity of rejecting this definition, or diſtinction of chemiſtry, and all others I have hitherto met with. Even Mr. Fourcroy's, though lately propoſed, does not appear to be well choſen or imagined. He defines chemiſtry "to be a ſcience "which teaches the knowledge of the intimate and reciprocal action "of all the bodies in nature on one another." To this definition it may be objected, that it requires much explanation. The intimate and reciprocal action are terms which cannot be readily underſtood. They would themſelves need new definitions to explain them, and to limit their meaning, and they might be the ſubjects of much diſputation *.

Since I find reaſon, therefore, to reject the definitions of others, it is incumbent on me to offer one which may be free from the objections which I have ſtated againſt them. And this I ſhall venture to attempt in theſe words:

Chemistry is the ſcience or ſtudy of thoſe effects and qualities of matter which are diſcovered by mixing bodies variouſly together, or applying them to one another with a view to mixture, and by expoſing them to different degrees of heat, alone, or in mixture with one another, in order to enlarge our knowledge of nature, and to promote the uſeful arts.

* When motion is given and taken away, by the colliſion of bodies, they certainly act intimately and reciprocally on each other: yet the ſtudy of this action is foreign to chemiſtry. Some further reſtriction is therefore neceſſary ere this definition be ſuitable to our ſtudy.

Or, in fewer words, *That the chemiſt ſtudies the effects produced by heat and by mixture, in all bodies, or mixtures of bodies, natural or artificial, and ſtudies them with a view to the improvement of arts, and the knowledge of nature* *.

This, in my opinion, takes in all that is proper to chemiſtry, and inſeparable from it, and at the ſame time leaves out every thing that does not belong to the ſcience.

It may, perhaps, be thought to leave out a great deal too much; and ſtrangers to it may find difficulty in conceiving how any great variety of curious or uſeful knowledge can be comprehended under this definition.

But when we ſhall have had opportunities to enter more fully into our ſubject, I have no doubt of its appearing clear, that not only an attention to the effects of heat and mixture, with the view I mentioned in the definition, is the characteriſtic of the chemical philoſopher, but that the ſtudy and ſcience thus defined lead to an inexhauſtible fund of intereſting or uſeful diſcoveries.

To form ſome idea of the views which this ſcience opens, and the objects which it preſents to the mind, we muſt reflect a little upon the ſubject of heat. Whatever that is which we call heat, and in whatever manner produced, it is certainly the chief material principle of activity in nature. Upon its gentle action, as confined within the bounds preſcribed to it in this part of the univerſe, depend the production and preſervation of all animal and vegetable life. Take away heat, to a certain degree, and they muſt all periſh: a total ſtop would be put to all the operations of nature. But beſide this extenſive influence, by which it ſupports action and life in this great ſyſtem of beings, the manner in which the various particular kinds of matter

* May it be thus defined? Chemiſtry is the ſtudy of the effects of heat and mixture, with the view of diſcovering their general and ſubordinate laws, and of improving the uſeful arts.

EDITOR.

are affected by the different degrees of heat, is a ſubject of inquiry which we ſhall find moſt fertile of ſurpriſing and uſeful diſcoveries.

Nor are the effects of mixture leſs various or extenſive than thoſe of heat. It is a field of inquiry to which we cannot ſee any bounds.

But, further, in order to apprehend better the nature and extent of this ſcience, it is neceſſary to obſerve, that the chemiſt does not confine his attention to the phenomena which he diſcovers himſelf by intended experiments: The knowledge he has acquired directs his attention to all ſimilar phenomena and operations which happen ſpontaneouſly, or in the courſe of nature, and enables him to reaſon better concerning theſe than any other perſon. Thus, various mixtures of ſubtile exhalations are oftentimes formed in the air, ſome of which produce meteors of different kinds, which draw our attention by their ſtriking appearance. Others, though not perceptible to the ſight, or other ſenſes, become but too manifeſt by their pernicious or fatal effects on various animals.

A perpetual ſucceſſion of mixtures and combinations is alſo going on in the waters of this globe, eſpecially thoſe of the ocean, and thoſe which flow through the hidden veins and caverns of the earth, where their qualities are often influenced alſo by the effects of ſubterranean heat.

Even the ſolid parts of the globe are undergoing conſtant changes of their mixture and compoſition, which give occaſion to new productions.

And, in animals and vegetables, their nouriſhment and growth, and the production of the different fluid and ſolid ſubſtances of which their bodies conſiſt, depend very much upon combinations of water with other matters, or upon changes of mixtures and combinations, which had been formed before.

On all theſe ſubjects, therefore, an intelligent chemiſt, on account of the knowledge he has acquired of the effects of mixture and heat, is the beſt judge. He is more ready to underſtand them, and to rea-

ſon upon them, than another perſon. And this has been one cauſe of the difficulty of giving a proper definition of chemiſtry. Some of thoſe who tried it thought it was neceſſary to comprehend in their definition all thoſe ſubjects concerning which the chemiſts had attempted to reaſon; and that all the qualities and phenomena which they had endeavoured to explain were proper and neceſſary objects of chemiſtry. But this was ſurely a very great error; for although chemical experiments have thrown ſome light on the digeſtion of the food in the ſtomach, we muſt not therefore conſider the ſtudy and knowledge of the digeſtion of our food as an article which belongs in particular to chemiſtry: the branch of ſcience to which it eſpecially belongs is the ſtudy of medicine. Some of the chemiſts have pretended to explain the virtues of moſt of the remedies employed by phyſicians, ſuppoſing them to depend on certain proportions which they contained of the imaginary principles, ſalt, ſulphur, water, earth, and others. Muſt we on this account admit that the ſtudy of the virtues of remedies does not belong to the phyſician but to the chemiſt? They alſo attempted to explain the phenomena of thunder and lightning, by ſuppoſing that nitrous and other vapours were elevated into the atmoſphere, and acted there on one another as we ſee them act in exploding compoſitions. Shall we, therefore, conſider the ſtudy of theſe meteors as a neceſſary part of chemical ſtudy or knowledge, although later diſcoveries have ſhown that the ſtudy of them belongs moſt particularly to the electrician?

The diſcoveries of the geometer have enabled him to explain many things in mechanics, in optics, in aſtronomy, and in the ſtructure of the bodies of animals; but we are not, therefore, to ſay that all theſe branches of knowledge belong to geometry, and make a proper part of it. The only ſtudy which belongs in particular to the geometer is that of the properties and relations of lines, figures, and quantities. The knowledge he acquires, by the ſtudy of theſe, proves a ſource from which we derive many clear explications of obſcure points, and

the ſolution of the moſt intricate queſtions in other ſciences; but all theſe other ſciences, as they are diſtinct from one another, are likewiſe ſo from the ſtudy which principally occupies and characteriſes the geometer.

Chemiſtry, therefore, muſt be underſtood to have the ſame relation to many other branches of knowledge that geometry has. It ſupplies principles by which many otherwiſe dark and intricate points in theſe other ſciences are clearly explained; and thus throws much light on many of the great operations of nature. But if we deſire to form a juſt judgment of the nature and limits of this ſcience, we muſt not conſider theſe applications of it to the ſupport and illuſtration of others, as eſſential parts of chemiſtry, or as parts which muſt be compriſed in its definition. In defining this ſcience, we muſt confine our attention to that body of knowledge which principally occupies and engages the chemiſt, and by the means of which he is enabled to throw light upon other ſciences more or leſs allied to his own *.

What is above ſtated will, I hope, ſerve to convey a preciſe idea of the extent and nature of chemiſtry, and at the ſame time ſhow that the chemiſt does not confine his attention to mere facts, but that his

* Is it not by a ſtrict attention to the limits allowed by this definition that we moſt clearly diſcover, in the organized bodies of this globe, principles of mutual relation and action altogether different from any diſcovered in the purely chemical phenomena? The hemlock and the pea ſpring from their reſpective ſeeds in the ſame water; the alum and the nitre ſhoot from their reſpective cryſtals in the ſame brine; and both go on increaſing with conſiderable ſimilarity. But, in the firſt example, a certain germ is neceſſary, and it grows by intus-ſuſception, and ſubſequent aſſimilation of heterogeneous matter, and protruſion of a new ſubſtance. In the ſecond, any particle whatever of each of the two ſalts may be the incipient cryſtal; and the apparent ſhooting is not a growth, nor a protruſion, nor the production of a new ſubſtance, but is an appoſition from without of a ſubſtance already preſent in the ſurrounding brine. The firſt is effected by a principle of *growth*, or *vegetable life*, producing two perfectly diſtinct individuals. The ſecond exhibits no ſuch principle; and the reſult is only a part of a maſs, which may increaſe till it contain all that exiſts of its kind in this globe. The mutual relations and laws of action are altogether diſparate and unlike.

EDITOR.

ſtudy is a ſpeculative and philoſophical ſcience, proceeding, like all other ſuch ſciences, on the relation of cauſe and effect.

If it be queſtioned, in the next place, upon what foundation it has been conſidered as ſo extenſively uſeful, we could eaſily give a liſt of the moſt uſeful arts which have had their origin from the obſervation of the effects of heat and mixture in various bodies. And as many of theſe arts employ for their chief materials ſubſtances the moſt familiar to the chemiſt, and the nature of which he knows better than any other perſon, it muſt be perceived that there is no ſcience more connected with the uſeful arts, or of greater conſequence to their improvement.

In order, however, to ſee this in a clearer light, let us take a curſory view of the chemical hiſtory of vegetables in general, and obſerve what particulars have been attended to, and how far the ſtudy of theſe has been uſeful. In the firſt place, by an attentive examination of vegetables, or vegetable ſubſtances, we have learned that ſome abound with reſin, others with gum; ſome contain ſweet juices, or ſugar, and others aſtringent, or bitter, or acid matter; in others we find oils of different kinds; and in others colouring or tinging materials, which can be transferred to wool, or linen, or other ſubjects of art; and many contain ſubſtances which have medicinal efficacy. All of them are either extracted or prepared for uſe by operations which are entirely or moſtly chemical, and which are therefore the foundation and ſupport of a variety of uſeful arts.

By applying water to vegetables, with a view to mixture, the chemiſt has learned that commonly ſomething is extracted and diſſolved by the water; and the application of heat to this water has ſhown that it commonly diſſolves more than without it. By expoſing this infuſion or decoction to a gentle heat, in open veſſels, he has found that the water thus gradually exhales and leaves the whole, or moſt of what it had diſſolved, in the form of a ſolid or tenacious maſs, applicable to the purpoſes of medicine; and a variety of others, accord-

ing to the nature of the vegetable. While the water exhales, he has often obſerved an odorous matter to ariſe with the vapour. This has ſuggeſted to him the contrivance of veſſels and inſtruments by which this vapour might be preſerved and condenſed again by cold in the apparatus for diſtillation. He has then diſcovered that the odour reſides in a ſubtile and volatile oil, which is thus ſeparated and preſerved; and the various kinds of which are uſeful in different arts, as well as in medicine.

The ſame operations, repeated with ſpirit of wine, inſtead of water, afford a variety of other products, many of which are equally uſeful.

By the ſimple application of fire, the vegetable is burnt to aſhes, the volatile parts being either diſſipated or conſumed. Theſe aſhes, being mixed with water, are diſſolved in part, and form a clear ley. When this clear ſolution, or ley, is decanted off, and expoſed to heat in open veſſels, the water exhales, and a ſalt remains behind, which has many uſeful qualities. Being mixed with ſand, and expoſed along with it to a violent heat, it produces glaſs, and is an indiſpenſable article in its compoſition. Upon this invention again depend the arts of enamelling, and glazing all kinds of earthen ware, the conſtruction of mirrors, teleſcopes, microſcopes, thermometers, and other philoſophical inſtruments, beſides the eaſy manufacture of veſſels, the moſt uſeful and elegant, for many of the purpoſes of common life. The ſame ſalt, properly mixed with oils, unites with them, and forms ſoap, and is the moſt active ingredient in that uſeful compound. It is likewiſe, both by itſelf, and in form of ſoap, an important material in the art of bleaching linen, and is often uſed in the art of dying, which is entirely chemical.

When the vegetable is expoſed to the action of heat, different kinds of ſteam, or vapour, are obſerved to ariſe from it before it takes fire. This has ſuggeſted the condenſation of theſe ſteams; and this again gave origin to the art of making tar, and to what is called the che-

mical analyſis of vegetables, by which, and by the further action of fire upon them, we learn a moſt curious and wonderful truth, which, however, agrees perfectly with what is obſerved in attending to their growth and nouriſhment. I mean that they are all compoſed of the ſame materials or principles, and theſe very few in number. Notwithſtanding the immenſe variety among them, or their parts, in point of form, colour, ſtrength or hardneſs, ſmell, taſte, medicinal efficacy, and other qualities, they can all be reſolved into a very few principles, or elementary ſubſtances, which are too ſimple to be ſuppoſed diſguiſed, and from whoſe various proportion, combination, and arrangement, therefore, the whole of this amazing variety is produced.

Inſtead of this outline of the chemical hiſtory of vegetables, we might have found an example equally good for our purpoſe in the hiſtory of the metals, the extraction of which from their ores, the refinement and ſeparation from one another, the giving them different degrees of hardneſs, elaſticity, and other properties, by which they become fit for many uſeful and important purpoſes, are all examples of the effects of mixture and heat. But to enter fully into any one of theſe articles would carry us at preſent a great deal too far.

It may perhaps be thought that I have ſaid too much in repreſenting chemiſtry as the ſtudy which has given occaſion to the invention and improvement of ſo many arts; and that many of thoſe arts have not received their origin or improvement ſo much from chemiſts as from other perſons, who, unacquainted with chemiſtry in general, but being employed in ſome chemical art, and hoping to find their intereſt in its improvement, engaged themſelves in inquiries and new trials, which conducted them to ſome uſeful diſcovery. But to this I would anſwer with what I ſaid before, that the nature of a ſcience, and what belongs to it, do not depend on an arbitrary name, or on the extent to which it is known and cultivated by any particular perſon, or at any particular time. It was certainly a part of chemiſtry which theſe artiſts were cultivating, although they might perhaps be unacquainted

with the reſt of the ſcience, and did not know that the ſtudy and reſearch in which they were employed belonged to chemiſtry. I can imagine a perſon living in a part of the world where the name of chemiſtry is totally unknown, but who, by his genius, taſte, and induſtry, in making experiments, might acquire a great part of the knowledge by which a chemiſt is at preſent diſtinguiſhed among us. His diſcoveries would not be the leſs a part of chemical knowledge that he did not know the name of this ſcience; nor is it to be doubted that he would deſerve the name of a chemiſt, and would even be conſidered as a very extraordinary chemical genius.

It is perhaps true that a greater number of improvements in arts have been invented by ingenious men who were artiſts themſelves, than by general and merely philoſophical chemiſts; but this is not ſurpriſing. The number of philoſophical chemiſts is ſmall, when compared with that of the chemical artiſts. And there are other reaſons why an ingenious and inquiſitive artiſt may often diſcover improvements in his art, eſpecially ſuch as are obvious and eaſy. The art to which he applies engroſſes the whole of his attention; and his intereſt conſtantly puſhes him on to attempt improvements, that he may vie with his rivals, and better his condition. He therefore becomes, in fact, a chemical philoſopher, as well as an artiſt, with this advantage, that his whole ſtudy is directed to one point, while the attention of the general and philoſophical chemiſt is divided among all.

The condition of an artiſt, therefore, is, in ſome reſpects, more favourable to the diſcovery of ſome improvements: but it would be incomparably more ſo, were he to acquire ſome general knowledge of the ſciences connected with his art. His field of knowledge would thus become more ample, and his views more extended; and his invention would have a larger ſtock to employ itſelf on, while, at the ſame time, the general principles of which he would become maſter would enable him to contrive new trials with better proſpect of ſuc-

ceſs, and to underſtand them more thoroughly when he had made them.

From this it is plain, that an acquaintance with the general principles of chemiſtry would be of the greateſt uſe and importance to many ingenious and inquiſitive artiſts, whoſe art is wholly or in part chemical.

Theſe few hints will, I hope, be ſufficient to anſwer the purpoſe I have at preſent in view, which is to engage your attention in the ſtudy of chemiſtry. And, as I have now alſo explained to you my idea of the nature and extent of this ſcience, you are fully prepared to enter upon it, and to underſtand the propriety of the plan which is to be obſerved in this courſe, and the great lines of which I ſhall now lay before you.

In the firſt place, the principal diviſion of our ſubject ſhall be into the *more general* and the *more particular doctrines* of chemiſtry.

Under the diviſion of more general doctrines ſhall be delivered,

1. An account of the more general or univerſal effects of heat.

2. The more general obſervations and diſcoveries relating to mixture.

3. An account of the chemical apparatus or inſtruments, and the manner of uſing them, or the chemical operations.

Under the diviſion of the more particular doctrines will be given

A particular account of all the moſt remarkable bodies, or kinds of matter, which the chemiſts have ſtudied; which ſhall be diſtributed into a number of claſſes, and conſidered in that order which, in my opinion, is beſt ſuited to their being eaſily underſtood and remembered *.

* I truſt that you will find that the account which I ſhall give of their properties is not merely a vaſt collection of individual facts, each of which muſt be remembered in itſelf, but that your own reflections will naturally form thoſe facts into general groupes or parcels, each individual of a claſs having a common character, which being recollected, they are all remembered. By this voluntary proceſs of your own thoughts, you will find yourſelves, at the end of our courſe, poſſeſſors of the knowledge of many extenſive

And, laſtly, while we thus give an extenſive view of the ſcience of chemiſtry, we ſhall not neglect the application of it to the illuſtration and improvement of pharmacy and other chemical arts. This is the ſecond great branch of my propoſed plan. It is plain that this will be in our power. For if ſcience be the diſcovery of the laws of nature, the knowledge of thoſe laws will enable us to foreſee what will be the reſult of any proceſs, and muſt point out to us, in all caſes, the means, and the beſt means, for producing any deſired chemical effect: and here does our ſcience repay, with a liberality unparalleled in any other ſcience, all her former obligations to the arts of life. From them did ſhe borrow the many facts which excited her to ſpeculate; and her occupation has at laſt enabled her to repay her debts with large intereſt, while ſhe has grown rich in knowledge almoſt beyond hope.

laws of nature, which regulate all the chemical operations on this globe. This will be the body of ſcience, founded on obſervation and experiment, which conſtitutes the reward of your attention and ſtudy.

GENERAL DOCTRINES OF CHEMISTRY.

PART I.

GENERAL EFFECTS OF HEAT.

INTRODUCTION.

OF HEAT IN GENERAL.

THAT this extenſive ſubject may be treated in a profitable manner, I propoſe

1*ſt*. To aſcertain what I mean by the word HEAT in theſe lectures.

2*dly*. To explain the meaning of the term cold, and aſcertain the real difference between cold and heat.

3*dly*. To mention ſome of the attempts which have been made to diſcover the nature of heat, or to form an idea of what may be the immediate cauſe of it.

4*th*, and laſtly, I ſhall begin to deſcribe the ſenſible effects produced by heat on the bodies to which it is communicated.

Any perſon who reflects on the ideas which we annex to the word heat will perceive that this word is uſed for two meanings, or to expreſs two different things. It either means a ſenſation excited in our organs, or a certain quality, affection, or condition, of the bodies around us, by which they excite in us that ſenſation. The word is

uſed in the firſt ſenſe when we ſay, we feel heat; in the ſecond when we ſay, there is heat in the fire, or in a hot ſtone. There cannot be a ſenſation of heat in the fire, or in the hot ſtone, but the matter of the fire, or of the ſtone, is in a ſtate or condition by which it excites in us the ſenſation of heat.

Now, in beginning to treat of heat and its effects, I propoſe to uſe the word in this ſecond ſenſe only, or as expreſſing that ſtate, condition, or quality of matter, by which it excites in us the ſenſation of heat. This idea of heat will be modified a little, and extended as we proceed, but the meaning of the word will continue at bottom the ſame, and the reaſon of the modification will be eaſily perceived.

All the experience we have relating to this quality or affection of matter ſhews, that it is the moſt communicable from one body to another of any quality that we know. Hot bodies cannot be placed in the contact or neighbourhood of colder ones, without communicating to theſe a part of their heat.

When a lump of hot iron is taken out of the fire, how can we prevent it from communicating its heat to the ſurrounding matter? Lay it on the ground, or on a ſtone, it very quickly communicates to them a part of its heat; lay it on wood, or any other vegetable or animal matter, it heats them in a very ſhort time to ſuch a degree as to ſet them on fire; let it be ſuſpended in the air by a wire, a little attention will ſoon convince us that it communicates heat very faſt to the air in contact with it.

Thus heat is perpetually communicated from hotter bodies to the colder around them, and, while it paſſes from the one to the other, it penetrates all kinds of matter without exception: Denſity and compactneſs are no obſtacle to its progreſs: It appears to paſs even faſter into denſe bodies, in moſt caſes, than into rare ones; but the rare and the denſe are all affected by it, and tranſmit it to others: Even the vacuum formed by the air-pump is pervaded by it. Sir Iſaac Newton firſt diſcovered this by an experiment. He ſuſpended an in-

ſtrument for meaſuring heat in a large glaſs veſſel, and exhauſted the air, and ſuſpending at the ſame time another ſimilar inſtrument in another glaſs veſſel, equal to the former, but not exhauſted, he perceived that the one was affected by the variations of heat as well as the other. (Newton's Optics, Query 18th.)

Much more lately ſome experiments on the ſame ſubject were made by the celebrated Dr. Franklin and ſome of his friends at Paris. They ſuſpended a hot body under the exhauſted receiver of an air-pump, and another ſimilar body, equally hot, in the air of the room near the air-pump, and theſe bodies being ſuch as to ſhew exactly the variations of heat that happened in them, it was perceived that both of them gradually loſt a part of their heat, until they were reduced to the temperature of the room in which the experiment was made, but that the one which hung in the air loſt its heat faſter than the one which was ſuſpended *in vacuo*.

The thermometers fell from 60° (Reaumur).

	IN VACUO.	IN THE AIR.
to 50°	in 17 min.	in 7 min.
37	54	22
30	85	29
20	167	63

The times of cooling are nearly in the proportion of 5 to 2. This is further confirmed by a ſet of ſimilar experiments, made by Sir Benjamin Thompſon. (Phil. Tranſ. for the year 1786.)

Sir Iſaac Newton thought that ſuch experiments gave a proof that the vacuum of an air-pump is not perfect, but that there is in it ſome ſubtile matter by which the heat is tranſmitted. This opinion probably was founded on a very general aſſociation in our minds, between the ideas of heat and matter; for, when we think of heat, we always conceive it as reſiding in ſome kind of matter; or poſſibly this no-

tion of Sir Iſaac might be founded on ſome opinion which he had formed concerning the nature of heat.

There is great reaſon however, independently of this experiment, for believing that the vacuum of an air-pump is not a perfect vacuum, and for thinking that there is always ſome ſubtile matter, or vapour, preſent in it; but I can eaſily imagine, and we ſhall afterwards ſee abundant reaſon to believe, that heat may be communicated, or paſs through a vacuum, or a ſpace empty of all other matter.

In this manner, therefore, and upon all occaſions without exception, is heat communicated from hotter bodies to colder ones, when they are in contact, or in the vicinity of one another; and the communication goes on until the bodies are reduced to an equal temperature, indicating an equilibrium of heat with one another.

When we conſider this communication of heat from hot bodies to colder ones, the firſt queſtion which may naturally occur to our mind, is, In what manner have theſe two bodies acted, the one on the other, on this occaſion? Has one of them loſt ſomething, which the other has gained? And which of them has loſt, or which has received?

The vulgar opinion is, that the hot body has loſt ſomething which has been added to the other. And thoſe who have attempted to reaſon more profoundly on the nature of heat, have agreed with the multitude in this point; and have ſuppoſed that heat is a poſitive quality, and depends, either upon an exceedingly ſubtile and active matter, introduced into the pores of bodies, or upon a tremor or vibration excited among their particles, or perhaps among the particles of a peculiar ſubſtance preſent in all bodies; which ſubtile matter, or tremulous motion, they have ſuppoſed to be communicated from the hot body to the colder, agreeably to our general experience of the communication of matter or of motion.

But although many philoſophers have thus agreed with the indiſtinct notion of the vulgar concerning heat, that it is a poſitive quality, or an active power reſiding in the hot body, and by which it acts

on the cold one; ſome of them have not been altogether conſiſtent in this opinion. They have not adhered to it, with reſpect to all the various caſes in which bodies of different temperatures act one on the other. They have ſuppoſed that, in ſome caſes, the colder body is the *active* maſs, or contains the *active* matter; and that the warmer body is the paſſive ſubject which is acted upon, or into which ſomething is introduced. When a maſs of ice, for example, or a lump of very cold iron, is laid on the warm hand, inſtead of heat being communicated from the warm hand to the ice, or cold iron, they have ſuppoſed that there is in the ice, or cold iron, a multitude of minute particles, which they call particles of froſt, or frigorific particles, and which have a tendency to paſs from the very cold bodies into any others that are leſs cold; and that many of the effects, or conſequences of cold, particularly the freezing of fluids, depend on the action of theſe frigorific particles. They call them Spiculæ, or little darts, imagining that this form will explain the acutely painful ſenſation, and ſome other effects of intenſe cold.

This, however, is the groundleſs work of imagination.

To form a well-grounded judgment on this ſubject, we muſt begin by laying aſide all prejudices and ſuppoſitions concerning the nature of heat and cold, and then propoſe to ourſelves this ſimple queſtion, From whence do theſe two ſeemingly diſtinct qualities of bodies originally proceed? Where are the ſources of heat and cold? It will immediately occur, that heat has a manifeſt ſource, or cauſe, in the ſun and in fires. The ſun is evidently the principal, and perhaps ultimately, the only ſource of the heat diffuſed through this globe. When the ſun ſhines, we feel that it warms us, and we cannot miſs to obſerve that every thing elſe is warmed around us. It is alſo plain that thoſe ſeaſons are the hotteſt, during which it ſhines the moſt, as well as thoſe climates which are the moſt directly expoſed to its light. When the ſun diſappears, the heat abates, and abates the more the longer his influence is intercepted. We muſt therefore acknowledge the ſun

as a manifeſt cauſe, acting on all the matter around us, and introducing ſomething into it, or bringing it into a condition which is not its moſt ſpontaneous ſtate. We cannot therefore avoid conſidering this new condition or heat, thus induced in the matter around us, as a poſitive quality, or real affection, of which the ſun is the primary cauſe, and which is afterwards communicated from thoſe bodies, thus firſt affected, to others.

But, after having formed this concluſion with regard to heat, where ſhall we find any primary cauſe or fountain of cold? I am ignorant of any general occaſion or cauſe of cold, except the abſence or diminiſhed action of the ſun, or winds blowing from thoſe regions on which his light has the weakeſt power. I therefore ſee no reaſon for conſidering cold as any thing but a diminution of heat. The frigorific atoms, and particles of froſt, which have been ſuppoſed to be brought by the cold winds, are altogether imaginary. We have not the ſmalleſt evidence of their exiſtence, and none of the phenomena, on account of which they have been ſuppoſed to exiſt, require ſuch a fiction in order to their being explained.

Some perſons, however, may perhaps ſtill find it difficult to diveſt themſelves entirely of the prejudice, that in certain caſes, cold acts in a poſitive manner. Such perſons may perhaps appeal to our feelings, which give us a ſtriking proof of the reality of cold as well as of heat. When we touch a lump of ice, we feel diſtinctly that it has a quality of coldneſs, as well as that hot iron has the quality of heat.

But let us examine what we mean by this quality of coldneſs. We mean a quality, or condition, by which the ice produces a diſagreeable ſenſation in the hand which touches it; to which ſenſation we give the name of cold, and conſider it as contrary to heat, and to be as much a reality. So far we are right. The ſenſation of cold in our organs is no doubt as real a feeling as the ſenſation of heat. But if we thence conclude that it muſt be produced by an active or poſitive cauſe, an emanation from the ice into our organs, or in any other

way than by a diminution of heat, we form a haſty judgment. Of this we may be convinced by ſeveral experiments. We can, for inſtance, take a quantity of water, and reduce it to ſuch a ſtate that it will appear warm to one perſon, cold to another, and neither warm nor cold to a third; the firſt perſon muſt be prepared for the experiment by bathing his hand in cold water immediately before; the ſecond, by bathing his hand in hot water, or by a feveriſh heat in his blood; and the hand of the third perſon muſt be in its ordinary natural ſtate, while the water with which theſe experiments are made is of a lukewarm temperature. Even to the ſame perſon, ſuch water might be made to appear warm, when felt with one hand, and cold, when felt with the other. We are therefore under the neceſſity of concluding from theſe facts, that our ſenſations of heat and cold do not depend on two different active cauſes, or poſitive qualities, in thoſe bodies which excite theſe ſenſations, but upon certain differences of heat between thoſe bodies and our organs. And, in general, every body appears hot or warm on being touched, which is more heated than the hand, and communicates heat to it; and every body which is leſs heated than the hand, and which draws heat from the hand which touches it, appears cold, or is ſaid to be cold. The ſenſation is in ſome caſes agreeable, and in others diſagreeable, according to its intenſity, and the ſtate of our organs; but it proceeds always from the ſame cauſe, the communication of heat from other bodies to our organs, or from our organs to them. What can we more reaſonably expect than that the ſenſation produced by the intro-ſuſception of the cauſe of heat, whatever that may be, will be different from the ſenſation that accompanies its emiſſion from our bodies? The ſenſations of hunger and repletion are equally diſtinct.

Beſide the uneaſineſs produced by the touch of very cold bodies, the freezing of water has induced many to believe the exiſtence of frigorific particles. Water, they imagined to be naturally, or eſſentially fluid, and to have its fluidity in conſequence of the round figure and

fine poliſh of its particles; and they thought that to give it ſolidity, ſome powerful agent muſt be employed, which can pervert it from its natural ſtate. They have therefore ſuppoſed the exiſtence of frigorific atoms, of angular, pointed, and wedge-like forms, which, being introduced among thoſe of the water, entangle, and fix them one with another.

But the whole of this too is imagination and fiction. We have not the leaſt proof that the particles of water are round, or any good reaſon for imagining that they have that form. An aſſemblage of ſmall round bodies, however ſmooth or poliſhed, would not have the properties which are well known in water; and the ſuppoſition, that fluidity is a natural or eſſential quality of water, is a great miſtake, occaſioned by our ſeeing it in theſe parts of the world much more frequently fluid than ſolid. In ſome other parts of the world, its moſt common or natural ſtate is a ſtate of ſolidity; there are parts of the globe in which it rarely or never is ſeen fluid; and the one or the other ſtate of the ſubſtance, as of all other bodies, depends on the degree of heat to which it is expoſed. Pure ice never melts but when we attempt to heat it above a certain degree; and if we cool pure water to the ſame degree, or below it, we are ſure to ſee it ſooner or later completely congealed.

On theſe two facts alone, however, the ſenſation we have of cold, and the freezing of water, has been commonly founded the belief of the exiſtence of frigorific atoms, among the greater number of thoſe who have thought proper to adopt ſuch an opinion.

But ſome of them have been influenced alſo by the effect of ſalts upon ice or ſnow. Many experiments have ſhewn, that certain ſalts, or ſtrong ſaline liquors, if they be added to ice or ſnow, occaſion theſe laſt to melt very quickly, and, at the ſame time, to become much colder; in conſequence of which, this mixture of ice and ſalts is employed occaſionally for freezing many liquids which cannot be frozen

by ordinary colds. The liquid which is to be frozen is put into a veſſel, and this veſſel is plunged into the mixture of ice and ſalt.

Theſe, and a few other facts which we ſhall afterwards conſider, are enumerated by Profeſſor Muſchenbroeck, among the reaſons which he gives for his belief of the exiſtence of frigorific or congealing particles; but they are not a good foundation for ſuch an opinion; and we ſhall in the ſequel have an opportunity to explain theſe facts, without having recourſe to ſuch a ſuppoſition.

We have, therefore, reaſon to conclude, that when bodies unequally heated are approached to one another, it is always the warmer or leſs cold body which acts on the other, and communicates to it a real ſomething, which we call heat. Coldneſs is only the abſence or deficiency of heat. It is the ſtate the moſt proper to common matter; the ſtate which it would aſſume were it left to itſelf, and were it not affected by any external cauſe. Heat is plainly ſomething extraneous to it: It is either ſomething ſuperadded to common matter, or ſome alteration of it from its moſt ſpontaneous ſtate.

Having arrived at this concluſion, it may perhaps be required of me, in the next place, to expreſs more diſtinctly this ſomething; to give a full deſcription or definition of what I mean by the word, heat in matter.

This, however, is a demand which I cannot ſatisfy entirely. Yet I ſhall mention by and by, the ſuppoſition relating to this ſubject, which appears to me the moſt probable. But our knowledge of heat is not brought to that ſtate of perfection that might enable us to propoſe with confidence a theory of heat, or aſſign an immediate cauſe for it. Some ingenious attempts have been made in this part of our ſubject, but none of them have been ſufficient to explain the whole of it. This however ſhould not give us much uneaſineſs. It is not the immediate manner of acting, dependent on the ultimate nature of this peculiar ſubſtance, or this particular condition of common matter, that we are moſt intereſted in; we are far removed as yet from

that extent of chemical knowledge, which makes this a neceſſary ſtep of farther improvement. We have ſtill before us an abundant field of reſearch in the various general facts or laws of action, which conſtitute the real objects of pure chemical ſcience, namely, the diſtinctive characters of bodies, as affected by heat and mixture. And, I apprehend, that it is only when we have nearly completed this catalogue, that we ſhall have a ſufficient number of reſembling facts to lead us to a clear knowledge of the manner of acting peculiar to this ſubſtance, or this modification of matter; and, when we have at laſt attained it, I preſume that the diſcovery will not be chemical but mechanical. It would, however, be unpardonable, to paſs without notice, ſome of the moſt ingenious attempts, which have had a certain currency among the philoſophical chemiſts.

The firſt attempt I think was made by Lord Verulam; next after him, Mr. Boyle gave ſeveral diſſertations on heat; and Dr. Boerhaave, in his lectures on chemiſtry, endeavoured to proſecute the ſubject ſtill farther, and to improve on the two former authors.

Lord Verulam's attempt may be ſeen in his treatiſe *De forma Calidi*, which he offers to the public as a model of the proper manner of proſecuting inveſtigations in natural philoſophy. In this treatiſe he enumerates all the principal facts then known relating to heat, or to the production of heat, and endeavours, after a cautious and mature conſideration of theſe, to form ſome well founded opinion of its cauſe.

The only concluſion, however, that he is able to draw from the whole of his facts, is a very general one, viz. that heat is motion.

This concluſion is founded chiefly on the conſideration of ſeveral means by which heat is produced, or made to appear, in bodies; as the percuſſion of iron, the friction of ſolid bodies, the colliſion of flint and ſteel.

The firſt of theſe examples is a practice to which blackſmiths have ſometimes recourſe for kindling a fire; they take a rod of ſoft iron, half an inch or leſs in thickneſs, and, laying the end of it upon their

anvil, they turn and ſtrike that end very quickly on its different ſides, with ſmart blows of a hammer. It very ſoon becomes red hot, and can be employed to kindle ſhavings of wood, or other very combuſtible matter.

The heat producible by the ſtrong friction of ſolid bodies, occurs often in ſome parts of heavy machinery, when proper care is not taken to diminiſh that friction as much as poſſible, by the interpoſition of lubricating ſubſtances; as in the axles of wheels that are heavy themſelves, or heavily loaded. Thick foreſts are ſaid to have taken fire ſometimes by the friction of branches againſt one another in ſtormy weather. And ſavages, in different parts of the world, have recourſe to the friction of pieces of wood for kindling their fires. A proper opportunity will afterwards occur for conſidering this manner of producing heat, with ſome attention.

The third example above adduced in the colliſion of flint and ſteel, is univerſally known.

In all theſe examples, heat is produced or made to appear ſuddenly, in bodies which have not received it in the uſual way of communication from others, and the only cauſe of its production is a mechanical force or impulſe, or mechanical violence.

It was, therefore, very natural for Lord Verulam to form his concluſion, as the moſt uſual: Nay, perhaps the ſole effect of mechanical force or impulſe, applied to a body, is to produce ſome ſort of motion of that body. This eminent philoſopher has had a great number of followers on this ſubject.

But his opinion has been adopted with two different modifications.

The greater number of the Engliſh philoſophers ſuppoſed this motion to be in the ſmall particles of the heated bodies, and imagine that it is a rapid tremor, or vibration of theſe particles among one another. Mr. Macquer alſo, and Monſ. Fourcroy, both incline, or did incline, to this opinion. I acknowledge that I cannot form to

myſelf a conception of this internal tremor, that has any tendency to explain, even the more ſimple effects of heat, or thoſe phenomena which indicate its preſence in a body; and I think that Lord Verulam and his followers have been contented with very ſlight reſemblances indeed, between thoſe moſt ſimple effects of heat, and the legitimate conſequences of a tremulous motion. I alſo ſee many caſes, in which intenſe heat is produced in this way, where I am certain that the internal tremor is incomparably leſs than in other caſes of percuſſion, ſimilar in all other reſpects. Thus the blows, which make a piece of ſoft iron intenſely hot, produce no heat in a ſimilar piece of very elaſtic ſteel.

But the greater number of French and German philoſophers, and Dr. Boerhaave, have ſuppoſed that the motion in which heat conſiſts is not a tremor, or vibration of the particles of the hot body itſelf, but of the particles of a ſubtile, highly elaſtic, and penetrating fluid matter, which is contained in the pores of hot bodies, or interpoſed among their particles: A matter, which they imagine to be diffuſed through the whole univerſe, pervading with eaſe the denſeſt bodies; a matter, which ſome ſuppoſe, when modified in different ways produces light, and the phenomena of electricity.

But neither of theſe ſuppoſitions were fully and accurately conſidered by their authors, or applied to explain the whole of the facts and phenomena relating to heat. They did not, therefore, ſupply us with a proper *theory* or *explication* of the nature of heat.

A more ingenious attempt has lately been made, the firſt outlines of which, ſo far as I know, were given by the late Dr. Cleghorn, in his inaugural diſſertation, publiſhed here on the ſubject of heat. He ſuppoſes, that heat depends on the abundance of that ſubtile fluid elaſtic matter, which had been imagined before by other philoſophers to be preſent in every part of the univerſe, and to be the cauſe of heat. But theſe other philoſophers had aſſumed, or ſuppoſed one property only belonging to this ſubtile matter, viz. its great elaſticity,

or the ſtrong repellency of its particles for one another; whereas, Dr. Cleghorn ſuppoſed it poſſeſſed another property alſo, that is, a ſtrong attraction for the particles of the other kinds of matter in nature, which have in general more or leſs attraction for one another. He ſuppoſes, that the common groſſer kinds of matter conſiſt of attracting particles, or particles which have a ſtrong attraction for one another, and for the matter of heat; while the ſubtile elaſtic matter of heat is ſelf-repelling matter, the particles of which have a ſtrong repulſion for one another, while they are attracted by the other kinds of matter, and that with different degrees of force.

This opinion, or ſuppoſition, can be applied to explain many of the remarkable facts relating to heat; and it is conformable to thoſe experiments of Dr. Franklin, and of Sir Benjamin Thompſon, quoted above. For, wherever there is but a very ſmall quantity of common matter, as in the vacuum of an air-pump, there we may expect to find the matter of heat exceſſively rarefied, in conſequence of its own very great elaſticity and ſelf-repellency, which, in this caſe, is little counteracted by the attraction of other matter.

A cold body, therefore, placed in ſuch a vacuum, is ſupplied more ſlowly with heat, or with the matter of heat, than when placed in contact with common matter in a denſer ſtate, which, by its attraction for the matter of heat, condenſes a much greater quantity of it into the ſame ſpace. And a hot body, placed in ſuch a vacuum, will retain its heat longer than in ordinary circumſtances, in conſequence of the ſcarcity of common matter in contact with it, by the attraction of which, its heat would be drawn off more quickly than if there were no other matter preſent but the matter of heat.

Such an idea of the nature of heat is, therefore, the moſt probable of any that I know; and an ingenious attempt to make uſe of it has been publiſhed by Dr. Higgins, in his book on vegetable acid, and other ſubjects. It is, however, altogether a ſuppoſition; and I cannot at preſent make you underſtand the application of this theory, or

the manner in which it has been formed; the greater number of you not being yet acquainted with the effects of heat, and the different phenomena which this theory is meant to explain, nor with ſome diſcoveries which preceded this theory, and gave occaſion to it.

Our firſt buſineſs muſt, therefore, neceſſarily be, to *ſtudy the facts* belonging to our ſubject, and to attend to the manner in which heat enters various bodies, or is communicated from one to another, together with the conſequences of its entrance, that is, the effects that it produces on the bodies.

Theſe particulars, when conſidered with attention, will lead us to ſome more adequate knowledge and information upon the ſubject—which again will enable you to examine and underſtand the attempts that have been made to explain it, and put you in the way to form a judgment of their validity.

When we attend to the effects produced by heat in the bodies to which it is communicated, we ſee that they are very various in the different kinds of matter. But there are ſome effects which are produced in all kinds, or in a great variety of bodies, in a ſimilar manner, or with ſuch inconſiderable variations, that the ſimilarity of its action is ſufficiently evident. This is true, eſpecially with regard to the ſimpler kinds of matter, ſuch as water, ſalts, ſtones, metals, air, and many others. Theſe ſimilar effects, produced by heat upon ſuch bodies of the more ſimple kind, may therefore be conſidered as the *general* effects of heat; and thus diſtinguiſhed from many which it produces on certain particular bodies only.

Theſe general effects of heat are, EXPANSION, FLUIDITY, VAPOUR, IGNITION, or INCANDESCENCE, and INFLAMMATION, or COMBUSTION.

SECT. I.—OF EXPANSION.

BY expanſion is meant an enlargement of the bulk of bodies, which may be obſerved when their heat is increaſed; while, on the contrary, there is a correſponding contraction when it is diminiſhed.

Of this effect of heat, we have the moſt extenſive experience, with reſpect to all the ſimpler kinds of matter in nature. The only ſeeming exceptions, are a very few bodies, which, while we vary their heat to make it riſe a little above, or fall a little below a *certain* temperature, ſuffer, in appearance, ſome irregular variations of their bulk, which do not agree with the general rule; but theſe irregularities are ſpecial to thoſe bodies, and to *thoſe particular* variations of their heat. When we expoſe the ſame bodies to equal variations of heat, but at a higher or lower temperature, they are affected in the general manner.

It may, therefore, be announced as one of the moſt general facts in chemiſtry, that all bodies are expanded by heat, and contracted by cold.

It is not neceſſary to ſatisfy you of this by a great number of experiments. You may be aſſured of the general fact, and I ſhall merely aſſiſt you to conceive it more diſtinctly, by a few examples. To chooſe theſe the more properly, we may remark, that matter appears to us always under three forms, or under forms intermediate between ſome of theſe three. We have it either, *firſt*, ſolid and hard in different degrees; or, *ſecondly*, in the form of a fluid like water, oil, quickſilver, or the like; or, *thirdly*, we have it in the form of an elaſtic fluid, or vapour, like air or the ſteam of water. To ſee an example of the expanſion of *ſolid and hard matter* by heat, we may take a cylinder of iron or braſs, one inch in diameter, and ſix or eight inches

long, and exactly equal in thickneſs from one end to the other, the ends being alſo flat or ſquare with the ſides; we muſt alſo provide a flat ruler of iron, with a round hole in it, that is one inch in diameter, and that will barely allow the cylinder to paſs through it; and the ſame ruler muſt have two projecting parts on the edge of it at a diſtance from one another, which is exactly equal to the length of the cylinder while it is cold. The cylinder being now made red hot, and tried in this ſtate, will be found too thick to paſs through the holes, and too long to fall in between the two projecting parts of the ruler; but, being again cooled, it will be found contracted to its former dimenſions.

An example of the expanſion of *fluid matter* may be had by putting ſome water, or oil, or ſpirit of wine, into a round or oval glaſs, which has a long and ſlender neck, and by filling the whole body of the glaſs only. If the glaſs be then warmed by ſetting it in hot water, while the heat penetrates into it, and into the fluid which it contains, this fluid will ſwell and riſe in the neck; and if the heat be again abſtracted by means of cold water, the fluid will return to its former bulk.

The third experiment may be made with air, confined in a bladder, and in ſuch quantity, that the bladder, while the air is cold, ſhall not be fully diſtended by it. If the air thus confined be gradually warmed, by warming the bladder before a fire, it will expand and blow up the bladder until it be fully diſtended; and being again cooled, it will be ſure to return to its former flaccid ſtate.

Such expanſions and contractions, therefore, are always a conſequence of the variations of heat in bodies. In whatever temperature of heat we make the experiment, if a body is made ſtill hotter, it is ſure to expand, or, if it be made cooler than that temperature, it is ſure to contract. If, for example, inſtead of heating the iron or braſs, it be made much colder than at firſt, with ice or ſnow, a nice menſuration of its dimenſions would ſhew that it had contracted, inſtead

of expanding; and after it is made red hot, and thereby expanded, were we to make it ſtill much hotter, it would ſuffer a ſtill greater expanſion, more or leſs, proportional to the heat it had received.

The expanſion produced by heat muſt, therefore, be underſtood to take place in all bodies, on every occaſion when their heat is increaſed, excepting only a few particular caſes, to be mentioned preſently.

There is, however, this difference, when the experiments are made with different kinds of matter, that they do not agree together with reſpect to the *quantity* of the expanſion or contraction which they ſuffer from the ſame increaſe or diminution of their heat. In ſolid bodies it is in general ſmall, and not perceptible without nice menſuration of their dimenſions. In fluids it is more obvious, as is ſeen in water; and in elaſtic fluids it is ſtill more remarkable. But in the different ſpecies of ſolids, fluids, or elaſtic fluids, it is exceedingly different, nor has any circumſtance that explains this variety been yet diſcovered. We cannot, therefore, form a judgment of the rate according to which any particular kind of matter expands, except by making experiments, to compare it with others in this particular. A number of experiments have accordingly been made by different authors, the moſt accurate of which are thoſe publiſhed by Mr. Ellicot in the Phil. Tranſ. vol. 39, and thoſe by Mr. Smeaton, Phil. Tranſ. vol. 48, and ſome by Mr. Berthoud, Eſſai ſur l'Horlogerie, ſecond edition, Paris 1786.

Before we proceed further, it may be here remarked that this effect of heat occaſions ſome bodies to crack, and break in pieces, when they are ſuddenly heated or cooled. The ſubſtances moſt liable to this accident are ſuch as are otherwiſe brittle, or which have not either flexibility or a ſtrong coheſion of their parts: ſuch are ſulphur, glaſs, bad earthen-ware, and even caſt-iron, which, though it has a ſtrong coheſion of its parts, is liable to ſplit, by reaſon of its want of flexibility. The manner in which this effect is produced by expanſion is

ſufficiently evident. It is liable to happen in theſe bodies when the heat is applied ſuddenly, and to ſome parts of them only, and when the parts to which the heat is applied are not exceſſively thin.

When, for example, heat is applied ſuddenly to a part of a glaſs veſſel that is not very thin, the heat expands that ſurface of the glaſs to which it is applied, and continues to expand it more, before it can penetrate to the other ſurface: as this laſt is not yet expanded, the glaſs is neceſſarily ſtrained, or ſtretched, as if a force were applied to alter its form; and although it withſtands this force to a certain degree, it cannot long withſtand it, on account of its brittleneſs: It is therefore ſplit or broken. And the ſame muſt happen to bodies of this kind, when, after being heated, they are ſuddenly cooled. In both caſes the fiſſure always begins in the coldeſt ſide.

If ſuch bodies were flexible, it is evident that their flexibility would preſerve them from breaking when ſuddenly heated or cooled, as they would eaſily bear that ſmall change of their form, which would be induced by the unequal expanſion or contraction of their parts; and accordingly, other metals which are flexible are not liable to this accident, as caſt-iron is. But glaſs, though very valuable to the chemiſts by many excellent qualities, is ſo liable to it, that they often ſuffer inconvenience and loſſes by this defect in glaſs veſſels. The way to avoid theſe inconveniences and loſſes, as much as poſſible, will be pointed out hereafter, in deſcribing the chemical veſſels and other parts of the apparatus.

To other artiſts, the expanſion of bodies by heat has proved uſeful on ſome occaſions. When it is neceſſary, for example, to bind pieces of work very ſtrongly with iron bands or hoops, ſuch as carriage-wheels, or very large veſſels employed by the brewer, or other artiſts, the purpoſe is eaſily attained by taking advantage of this power of heat. The iron-hoops are made red hot, and driven on ſuddenly, while thus extended and widened by their expanſion, and then they are ſuddenly cooled by throwing cold water on them: this makes

them contract again and bind the work with ſuch very great force, that, on many occaſions, they make a deep print in it.

Another ſet of artiſts finding that this power of heat affected ſome nice machines which they are employed in conſtructing, and prevented theſe from having the degree of perfection deſired in their effect, have invented contrivances by which expanſion is made to counteract itſelf, or to remedy thoſe very defects which it occaſioned in ſuch machines. Theſe artiſts are the clockmakers and watchmakers. It was perceived that the going of common clocks and watches is very ſenſibly affected by the variations of heat, occaſioning the expanſion or contraction of ſome of their parts which are intended to regulate their motion. In the common clock, the rate of its going is regulated by the length of its pendulum. When the pendulum is ſhortened, the clock goes faſter; when it is lengthened, it goes ſlower; and as it muſt neceſſarily be lengthened by heat, and ſhortened by cold, a common clock, that goes at a proper rate in a moderate temperature of heat, will go too ſlow in a warmer one, and too faſt in a colder. This has been remedied very ingeniouſly by different contrivances, of one of which I ſhall here give the moſt ſimple idea.

Suppoſe that we have two metals, one of which expands or contracts juſt three times as much as the other, by the ſame variation of heat. They may, in the following manner, be employed in the conſtruction of a pendulum, that ſhall neither be lengthened by heat, nor ſhortened by cold.

From the point of expanſion A (fig. 1.) a rod or thick wire, A B, of the leſs expanſible metal, muſt hang down a certain length. At the lower end it muſt have a ſtud, or croſs piece, B C, ſtrongly faſtened, and projecting a little to one ſide. On the projecting part, C, of this croſs piece, muſt be erected a pillar, C D, of the more expanſible metal. To the top of this pillar, another croſs and projecting piece, D E, muſt be ſtrongly faſtened; and, from this laſt, muſt again hang down another rod or wire, E F, of the firſt metal, having the ball of

the pendulum at its extremity. And now, if the height of the pillar C D be one-third of the length of the two rods taken together, the pendulum can neither be lengthened by heat nor ſhortened by cold. For, by the expanſion of the pillar, the pendulum is ſhortened, or the ball is raiſed nearer to the point of ſuſpenſion, becauſe the upper end D of the pillar is more raiſed by its expanſion, than the lower end C is depreſſed by the expanſion of A B; and, on the other hand, by its contraction, the pendulum is lengthened, or the ball is lowered; but, while this happens, the two rods, by their expanſion or contraction, produce a contrary effect; and the quantity of expanſion or contraction is the ſame in the rods that it is in the pillar, the greater length in the rods compenſating for the greater expanſibility in the pillar. The conſequence therefore muſt be, that the length of the pendulum, that is, the diſtance between the point of ſuſpenſion and the ball, cannot be varied by heat or cold. Accordingly, the clocks made for the uſe of aſtronomers, have pendulums conſtructed upon this principle, in which pillars of the more expanſible metals are employed to counteract the expanſion of the other parts of the pendulum-rod.

I already remarked, that there are a few ſeeming exceptions from the general fact of the expanſion of bodies by heat, and their contraction by cold. Theſe are now to be taken notice of.

The moſt obvious and remarkable example of ſuch an exception occurs in water. This fluid, in paſſing through all the variations of heat, between the greateſt which it can bear, and a cold approaching to that by which it is frozen, is expanded or contracted like other fluids. But, during its change from the ſtate of a fluid to that of ice, inſtead of contracting by the diminution of its heat, it ſuffers a remarkable expanſion, becoming more bulky by about one-eighth, and that with a force which is almoſt irreſiſtible. It is common to ſee bottles burſt by the freezing of water within them. The ice, being firſt formed in the neck of the bottle, ſhuts it up cloſe; and the reſt of the water, while it afterwards freezes, is ſure to burſt the bottle by its expanſion.

But experiments have ſhewn that this expanſion of freezing water is performed with a force much greater than that neceſſary for the burſting of bottles. Mr. Boyle found that water, confined by a moveable plug or ſtopper, in a ſtrong braſs tube, three inches in diameter, lifted, while it froze, a weight of 74 lb. with which the ſtopper was loaded (Hiſtory of Cold). Huygens burſt an old cannon by freezing water confined within it (Du Hamel Hiſt. de l'Acad. Roy. l. i. § 2. ch. 1.) The academicians of Florence burſt a ſmall hollow braſs globe, the cavity of which was one inch in diameter, by filling it with water, which was afterwards frozen: And Profeſſor Muſchenbroeck has computed that the force neceſſary to the burſting of this ball muſt have been equal to a preſſure of 27720 lbs. weight. In the progreſs of this experiment, however, it appeared that the expanding force of this quantity of freezing water had not power to burſt a ſtronger ball (Muſchenb. Tentam. Florentina, p. 135). See experiments on the expanſive force of freezing water, made by Major Williams at Quebec, in the years 1784 and 1785, Phil. Tranſ. of Edin. vol. ii.

This ſtrong expanſive force in freezing water explains many things that happen in froſt, or are conſequences of it; ſuch as the burſting of water-pipes, the raiſing of pavement, the increaſe of fertility in the ſoil, and the ſplitting of trees, and even of rocks in ſome caſes. The gradual decay and demolition of neglected buildings alſo, and of the more elevated and rocky parts of the earth's ſurface, are promoted by the ſame cauſe. The burſting of water-pipes happens when they are too much expoſed to the cold, or not ſufficiently covered with earth, or other matter, to preſerve the water in them from freezing. The raiſing or looſening of the ſtones in the pavement, is occaſioned by the freezing of the humidity contained in the ſand or earth in which the ſtones are imbedded. The increaſe of fertility in the ſoil is a conſequence of the diſunion of all the parts of it, which are liable to cohere too ſtrongly together, and to give it a degree of toughneſs and denſity

not penetrable by the fine fibres of the roots of plants; but, while the humidity which is in ſuch ſoil is changed into ice, being every where interpoſed between the particles of earth, it diſunites theſe by its expanſion, and, when the thaw comes, the whole maſs is much leſs coherent and more eaſily penetrable.

The other effects of froſt above enumerated, ſome of which are principally obſerved in very cold countries, all depend on the ſame circumſtance, the expanſion of freezing water, or watery fluids, in the pores, or crevices, or natural cavities, of various bodies.

As this expanſion of freezing water has long been perceived, ſome attempts have been made to explain it, or to diſcover the cauſe of it. It appears at preſent to depend on two cauſes.

One of theſe is the extrication of the air which water contains, combined with it, in a denſe non-elaſtic ſtate. This air can be extricated from the water, by the uſe of the air-pump, and by heat, and, when thus ſeparated, always aſſumes the elaſtic form of common air. We can alſo perceive that it is ſeparated from the water during its congelation. While water is congealing, numerous bubbles of elaſtic air are formed within it, ſome of which riſe to the ſurface, and eſcape, ſo long as it is not entirely covered with ice; but thoſe produced after this happens, are entangled in the ice, and form thoſe numerous cavities commonly ſeen in it. A maſs of ice, therefore, with theſe cavities in it, muſt be more bulky than the maſs of water from which it was made. And this, accordingly, is one cauſe of the expanſion of freezing water in ordinary circumſtances. But experiments have ſhewn that it does not depend on this cauſe alone. Water has been deprived of its air, by the air-pump, and by heat, as much as poſſible, and yet, when frozen, was ſenſibly expanded. Mr. Mairan, who wrote a treatiſe on ice, is of opinion, therefore, that another cauſe contributes to the expanſion of water in freezing; and this other cauſe which he endeavours to aſcertain, is a ſtrong tendency of the parts of the water, to arrange themſelves into ranks and lines, which croſs one another at

angles of 60 and 120 degrees. He proves the exiſtence of ſuch a tendency in the parts of water, while they concrete into ice, by many examples of it. It may be perceived in water freezing in a common baſon, or other ſuch veſſel; the ice firſt formed, is in oblong, ſlender, pointed concretions, at the ſurface of the water, and adhering to the ſide of the baſon by one end. While theſe increaſe in length and ſize, others ſhoot out from the edges of them, at the angles above mentioned; and from theſe a third order, proceeding from the ſides of the ſecond, may perhaps be obſerved. Other ſimilar concretions, or thin plates of ice, are alſo formed, which point downwards; and the water, after being traverſed by a multitude of theſe ſpicular concretions and plates of ice, comes at laſt to be totally frozen. Now theſe appearances ſhew plainly, that the parts of the water are diſpoſed to cohere together in one particular manner, and to aſſume one arrangement in preference to others, when they concrete. Were they equally diſpoſed to unite in every way, and in every direction, the ice firſt formed would be a ſmooth cruſt of equal thickneſs, adhering to the ſides or internal ſurface of the baſon; and, in the farther progreſs of the congelation, this cruſt of ice would increaſe in thickneſs, until the whole water were changed into ice. Another proof of this tendency of the parts of water to arrange themſelves in a particular manner, may be had by examining the flakes of ſnow, while it falls in very cold weather. Theſe are evidently formed by the concretion of the parts of water into ice in the atmoſphere, where there is no obſtacle to prevent their concreting together, in any manner to which they are the moſt diſpoſed. When the weather is ſo cold that we can examine theſe flakes of ſnow before any part of them melt, we find them generally compoſed of a number of fine needles, or ſpicular concretions, ſometimes irregularly collected together, but, at other times, joined into regular plane figures, reſembling a ſtar of ſix rays, the angles between which are exactly equal. On many occaſions, the rays of theſe ſtarry figures have had ſmall branches iſſuing from their ſides, at angles equal to

thoſe which the rays themſelves formed with one another, and have been otherwiſe compounded and varied (Muſchenbroeck Phyſ. de Meteoris). Some authors have concluded, from theſe figures, that ſuch ſnow was not formed from pure watery vapours, but from vapours or clouds which contained an admixture of ſaline, or other particles, which gave the diſpoſition to form ſuch figures. But this opinion is not conſiſtent with the experiments that have been made with ſnow, which, when collected pure, and melted into water, gives a water purer than any other natural water. Theſe forms of the ſnow muſt, therefore, be the effect of a property natural to pure water, and they are moſt regular when the ſnow falls in very cold weather, and with little or no wind. The atmoſphere being then calm, does not hinder the water, or watery vapours that are in it, to concrete in the manner to which they are the moſt diſpoſed.

To underſtand more diſtinctly how the parts of water may have this tendency to concrete in this particular manner, we may imagine that it depends on ſome degree of polarity, with which the attraction for one another is attended. There is no doubt that the immediate cauſe of their concretion is their attraction for one another; and if we ſuppoſe that this attraction is modified by ſome ſort of polarity, which diſpoſes the parts of the water to cohere together, by ſome of their ſides in preference to others, they will naturally form rectilineal figures, and ſpicular maſſes, joined together at certain angles, ſuch as would be formed by a great number of little balls of cork, if a ſmall ſteel magnet were thruſt into each of them, and the whole of them thrown into a tub of water. But, whatever may be thought of this ſuppoſition, there is no room to doubt, that one reaſon of the ſwelling of water, when it is changed into ice, is, that the parts of it enter ſuddenly into a mode of arrangement and connection, different from that of the parts of the water in its liquid ſtate, and which requires more ſpace, in the proportion of about nine to eight.

I muſt here obſerve that this exception to the general contraction

of ſimple ſubſtances, by the diminution of their heat, is not confined to the mere paſſage of water into the ſolid form of ice, but begins to be obſerved while the water is yet a ſmall degree warmer than ice. It ceaſes to contract, and while cooling down to the temperature of ice, it expands about $\frac{1}{172}$th of its bulk. This was firſt obſerved (I think) by Mr. Baumé of the French Academy, and is mentioned by him in the accounts which he gives of his hydrometer, and of diſtillation. Mr. de Luc has examined this matter with ſtill more attention; and Count Rumford has pointed out, with much ingenuity, ſome important conſequences of this ſingularity, in the great operations of nature. (See Count Rumford's Eſſays, vii. 281, &c.)

The only other examples of exception from the general fact of expanſion by heat have been obſerved in fuſible iron, and one or two other metals; and they are very ſimilar to that of water above deſcribed. Theſe metals are expanded by heat, and contracted by cold, in every variation of heat to which they are commonly expoſed. But when we heat them ſo much as to make them melt, the metal, while melting, inſtead of being expanded by the heat then communicated to it, is ſenſibly contracted, and forms a fluid denſer than the ſolid was immediately before; a conſequence and teſt of which is, that any ſmall portion of the metal that yet remains unmelted, ſwims at the ſurface of the melted metal, as ice does in water. This ſingularity is obſerved in iron, antimony, and one or two more of the brittle metals. In theſe ſeveral caſes, certain other appearances preſent themſelves ſimilar to thoſe obſerved in the caſe of water. The parts of the metal evidently aſſume a particular arrangement, while they are congealing, in conſequence of which they concrete into a number of oblong, ſpicular, or angular maſſes, or branched figures, very conſpicuous in antimony, biſmuth, and zinc, when they are broken, and perceptible in caſt-iron alſo. We may therefore ſuppoſe that the attraction by which their parts are made to cohere is alſo attended with ſome degree of polarity, which forces them to aſſume a determined relative poſition,

and a new and more bulky arrangement, in paſſing from a ſtate of fluidity to that of ſolidity.

Having now deſcribed the few exceptions from the general fact of the expanſion of bodies by heat, and their contraction by cold, it is proper to mention a remarkable circumſtance with which it is attended. The increaſe of bulk produced by heat is in many caſes ſo conſiderable, that it might naturally produce a ſuppoſition that the weight of the body is alſo increaſed. But, when we examine the fact by experiment, we find it quite otherwiſe.

Some experiments for this purpoſe are circumſtantially related by Dr. Boerhaave in his Elements of Chemiſtry. They were made with a maſs of iron, of the weight of five pounds, and repeated afterwards with other metals, which being weighed red hot, and after they were again cold, never ſhewed any variation of their weight, but what was ſo trifling and uncertain, that the Doctor concludes there was not in truth any variation, but only ſmall appearances of it, proceeding from errors or miſmanagement of the balance.

Profeſſor Muſchenbroeck, however, was not ſatisfied with theſe experiments of Dr. Boerhaave. He was ſtrongly inclined to believe that heat is ponderous, or produced by a ponderous ſubſtance. It appears alſo that Mr. Buffon thought he could ſhew by experiments that a body is heavier when it is hot than when it is cold. But ſimilar experiments being made more lately in England, by Dr. Roebuck, (Phil. Tranſ. vol. 66.) and alſo by Mr. Whitehurſt (ibid.), who is diſtinguiſhed by his accuracy and ingenuity in the conſtruction of very nice and delicate balances, they diſagreed entirely with thoſe reported to have been made by Monſ. Buffon. Dr. Roebuck made his experiments with the moſt ſcrupulous accuracy, and on conſiderable maſſes of iron, iron-ſcales, copper, and ſilver; and Mr. Whitehurſt made his with a ſmall maſs of gold, weighed with an exceedingly nice and delicate balance. In all theſe experiments the bodies appeared heavier cold than when they were hot; and one reaſon for this is obvious:

the hot body when placed in one of the ſcales of the balance, heated and rarefied the air over that ſcale, which being therefore leſs preſſed down by the air over it than the other ſcale, this laſt preponderated. More recent experiments, by Dr. Fordyce, in which this cauſe of error was ingenioufly avoided, concur to ſhew that bodies become heavier, though in a very ſmall degree only, not by the increaſe, but by the diminution of their heat. Theſe experiments are to be conſidered in the ſequel.

It is therefore evident, that if heat depends on the preſence of a ſubtile matter introduced into bodies, this matter has not any perceptible degree of gravitation.

But it would be improper to omit the mention of one other fact which Muſchenbroeck and others thought to be a ſtrong proof of the opinion that heat is a gravitating ſubſtance. This fact was diſcovered in making experiments with the metallic ſubſtances. Many of theſe, when expoſed to the action of heat for ſome time, are gradually calcined, or changed into a calx, which, ſo long as it remains in that ſtate, is an earthy-like ſubſtance, which has none of the metallic properties. And the fact above alluded to is, that the weight of the calx is greater than that of the metal from which it was produced: 100 pounds of lead yield 110 or 112 pounds of calx, and other metals give a ſtill greater increaſe of weight in their calxes.

But this increaſe of weight in a metallic ſubſtance by calcination is no proof that heat is ponderous. When the metallic calx is removed from the fire, the heat leaves it as it would leave any other matter. It muſt therefore be ſomething elſe than heat which gives this increaſe of weight; and later diſcoveries have well aſcertained that it is air, a quantity of which is attracted and fixed by the calcining metal, and can again be extricated from it. No metal can be calcined without the plentiful admiſſion of air to it, while it is expoſed to the action of heat.

It has not, therefore, been proved, by any experiment, that the

weight of bodies is increaſed by their being ſimply heated, or by the preſence of heat in them.

This may be thought very inconſiſtent with the idea of the nature or cauſe of heat, which I lately mentioned as the moſt plauſible: I mean the notion that heat depends on the abundance of a ſubtile matter highly elaſtic, or ſelf-repellent, which eaſily enters into all bodies, and penetrates them throughout, being ſtrongly attracted by the matter of thoſe bodies.

It muſt be confeſſed that the above fact may be ſtated as a ſtrong objection againſt this ſuppoſition.

Some have attempted to remove it, by ſuppoſing the matter of heat to have ſuch a very high degree of ſubtility and tenuity, that no quantity of it that we can collect together can have any ſenſible weight. Others have thought it might be the matter which cauſes the gravitation of other bodies, and that it could not be ſuppoſed to gravitate like them, but, on the contrary, might have an oppoſite tendency.

Theſe attempts to remove the objection are ingenious, but they are not ſatisfactory. We find too much difficulty in attempting to comprehend them. And this has contributed more than any direct arguments, to confirm in their opinion thoſe, who, with Lord Verulam, aſſert that heat, or the cauſe of our ſenſation of heat, and all the phenomena which accompany that ſenſation, is not a material ſubſtance, transferable from one body to another, but a mere ſtate or condition, in which the matter of all bodies may be found. Yet, notwithſtanding this difficulty, I imagine that, as we proceed, you will find yourſelves more and more impreſſed with the belief that heat is the effect of a peculiar ſubſtance.

The non-ponderoſity of heat being, however, aſcertained by all the experiments hitherto made, it is neceſſarily attended with this obvious conſequence, that the denſity of bodies, in compariſon with one another, or what is called their ſpecific gravity, is conſiderably affected by the variations of heat. As experiments have ſhewn that each kind of

matter has its own peculiar rate of expanding, or differs from others in the quantity of expanſion which it ſuffers, by a certain variation of its heat, it neceſſarily follows, that when we increaſe or diminiſh the heat of a number of different bodies in a ſimilar manner, we change the properties of their denſities to one another. In the tables of the ſpecific gravity of different bodies, therefore, theſe ſpecific gravities are ſtated as they appear in a moderate temperature of heat, the degree of which ought always to be marked at the head of the table. In higher or lower temperatures, it is well known that the ſpecific gravities of the ſame bodies would be differently proportioned to one another.

We have now ſufficiently conſidered *Expanſion* as an effect of heat, and taken notice of the few exceptions to it.

Our next employment ſhall be to explain the nature of the thermometer, which was invented in conſequence of the diſcoveries already deſcribed, and which has proved exceedingly uſeful, both in chemiſtry and medicine, and has enabled us greatly to enlarge and improve our knowledge of heat.

OF THE THERMOMETER.

The hiſtory of this invention is a little obſcure, and the contrivance of the firſt thermometer has been attributed to three or four different perſons. Sanctorio, who is diſtinguiſhed by the diſcovery of what is called inſenſible perſpiration, appears, however, to have the beſt title to it.* It is uncertain what purpoſe he had in view when he contrived the thermometer which ſtill bears his name; but it does

* This title however, is ſomewhat dubious. Drebel, a phyſician at Alkmaer in Holland, certainly made many thermometers at this time (the beginning of the 17th century) and they were very common in Holland, and even in England, before Sanctorius was known in theſe countries. Robert Fludd, alſo, who began to publiſh his alchemical writings in 1597, ſpeaks a great deal about thermometers, and of many purpoſes to which he applied them. Much of this indeed is unintelligible, or is ſtark nonſenſe—but he muſt have had the conception of ſuch an inſtrument.

EDITOR.

not appear that the expanding power of heat had been known, or attended to, before his time; and, having obſerved it in air, he thought of employing it as a meaſure of heat, by the following ſimple contrivance, (fig. 2.)

A glaſs tube, open at one end, is blown up into a ball at the other. The ball is warmed, and the tube is then ſet upright, with its open end dipped into a ſmall ciſtern of any coloured liquor. When the ball is warmed by the approach of a burning coal, the air in it is expanded by the heat, or becomes more elaſtic; a part of it therefore iſſues out at the open extremity of the tube, and riſes up through the coloured liquor in bubbles. Let the coal be now removed, and the ball allowed to cool again to the common temperature of the place. While it cools, the air in it loſes the additional elaſticity which it had received from the heat, and the preſſure of the atmoſphere compreſſes it into a ſmaller bulk than that it had while it was warm, and cauſes ſome of the coloured liquor to aſcend in the tube.

A ſcale of equal degrees, applied to the whole length of the tube, ſerved to divide its cavity into a number of ſmall and equal ſpaces, and it is now eaſy to ſee by how many of theſe ſmall and equal ſpaces the bulk of the air was increaſed in conſequence of its being heated, or diminiſhed in conſequence of its being cooled. This contrivance, therefore, was found to be a real thermometer, or an inſtrument which ſhewed the increaſe or diminution of heat in the place in which it was kept.

The power of heat to expand air being thus perceived, a curioſity was ſoon excited to try other fluids, as well as ſolid bodies, and the reſult was what has been already delivered. It was diſcovered that all bodies expand when their heat is increaſed, and that other fluids might be employed, as well as air, in the conſtruction of thermometers. It was even very ſoon perceived that air was leſs fit for this purpoſe than any other fluid, for this reaſon, that being in this inſtrument conſtantly ſubjected to the preſſure of the atmoſphere, acting on it through the

open end of the tube, it might be contracted or expanded in bulk, in conſequence of variations in that preſſure alone, although its heat continued the ſame. This defect in the air thermometer was perceived by Mr. Boyle, and by the Florentine Academicians, nearly at the ſame time, and both thought of employing other fluids.

Spirit of wine was the firſt choſen. It expands or contracts much more than water by the ſame variation of its heat, and is not liable to freeze like that fluid in violent colds, and it is eaſily tinged, to make it more viſible in the tube.

After ſpirit of wine had been uſed for ſome time, quickſilver was alſo found to be very fit for this purpoſe; and whichever of theſe fluids were choſen, they were put into a glaſs tube, blown up at one end into a ball, and as much fluid put in as filled the ball and a part of the tube. By this ſimple contrivance, the ſmalleſt expanſions and contractions of the fluid are eaſily diſcernible, and eaſily diviſible into ſmall parts, or degrees. The extremity of the tube is cloſed, by melting the glaſs, to prevent any loſs of the fluid by evaporation, or any change which might happen in it by the action of the air. Some authors had ſcruples at firſt about this practice of cloſing up the tube: It was apprehended that the air confined in the upper part of it would reſiſt the aſcent of the expanding fluid: But it is eaſy to expel a great part, or the whole of this air, before the tube is cloſed up; nor is it at all inconvenient to leave a part of it; experience having ſhewn that the expanſion of the fluid is performed with a force too ſtrong to be reſiſted by the elaſtic preſſure of the air, eſpecially when that air is rarefied by the abſtraction of a part of it.

In a thermometer conſtructed in this manner, the fluid cannot be affected by any cauſe but heat alone; and the inſtrument is therefore free from the defect of Sanctorio's thermometer. But for a conſiderable time after it was brought to this ſtate, it ſtill remained very imperfect. No method had yet been thought of for making thermometers agree with one another. The ſcale of degrees applied to the tube was

differently conſtructed and differently applied in every thermometer; and experiments made with one could not be uſefully compared with thoſe made with another. The moſt important improvement of theſe inſtruments, therefore, ſince they were firſt contrived, was the invention of a method of conſtructing and applying their ſcales, ſo as to make them agree together when they are expoſed to the ſame temperature of heat, whatever that be.

This was attempted by different methods, more or leſs ſucceſsful, and ſome of which were practiſed for a conſiderable time in ſome of the countries of Europe, until one was diſcovered preferable to all the reſt. This is now become the univerſal rule for the graduation of theſe inſtruments.

It will therefore be ſufficient here to explain this method alone, the advantages of which, in compariſon with the others, are very well ſtated by Dr. Martin (Martin's Eſſays on Heat and Thermometers).

It was eaſily diſcovered by the experiments of Mr. Boyle, and others, that ſome bodies which are liable to changes of their form, in certain variations of their heat, undergo theſe changes at a certain degree of heat, or when we attempt to increaſe or diminiſh their heat above or below that particular degree *. Therefore, if a thermometer be applied to them, while they are undergoing the change, the fluid in it is always reduced to the ſame ſtate of expanſion. We have an example of this in the freezing of water, and in the melting of ice and ſnow. If, for example, I take a thermometer of quickſilver, and put it into melting ſnow, taking care that the ball be entirely covered with the ſnow, the quickſilver will be contracted by the cold, and will deſcend in the tube; but it will ſoon ſtop and deſcend no further, but will continue in the ſame ſtate ſo long as any conſiderable part of the ſnow remains unmelted. If I now mark with a diamond, or with a ſmall file, that part of the tube at which the quick-

* This was diſcovered, with reſpect to the freezing of water, in 1664, by Dr. Robert Hooke. EDITOR.

ſilver ſtopped, and afterwards repeat the experiment with the ſame thermometer, however often, and at places and times the moſt diſtant, the reſult will be always exactly the ſame; the quickſilver in the melting ſnow will deſcend to the ſame part of the tube to which it deſcended the firſt time, and will remain there ſtationary ſo long as any conſiderable part of the ſnow remains unmelted.

This ſhews, that melting ſnow is always equally cold, or has the power to reduce the quickſilver of a thermometer to one ſteady determined ſtate of expanſion or denſity, which may be called the melting-ſnow expanſion of the quickſilver.

A ſimilar power has been obſerved alſo in boiling water *. If the ſame thermometer uſed in the above experiments, be now immerſed into boiling water, and the water be kept boiling around it for ſome time, the quickſilver, heated and expanded, will aſcend in the tube, but it will aſcend to a certain height only; however long or violently the water is boiled, it will not aſcend higher. If we now mark with a diamond or file, that part of the tube to which the quickſilver roſe, and afterwards repeat the ſame experiment, with the ſame thermometer, ever ſo often, in places not very high above the ſurface of the ſea, the reſult in this caſe alſo will be always the ſame; the quickſilver will always riſe to the ſame point which was marked the firſt time, or with very little variations, which can be foreſeen, and allowance made for them. Thus we learn, that boiling water, when in the ſame circumſtances, is always equally hot, or has the power to reduce quickſilver to another determined and ſteady ſtate of expanſion, which may be called the boiling-water expanſion of quickſilver. And, now, having acquired this information, I can repeat the ſame operations with any other thermometer; I can find and mark the part of the tube to which the quickſilver deſcends, when reduced to its melting-ſnow ſtate of expanſion, and the point to which it aſcends,

* This alſo was the diſcovery of Hooke in 1684. EDITOR.

when reduced to its boiling-water ſtate of expanſion. The diſtance between theſe two points on the tube, will be very various in different thermometers, on account of their different ſize, and the different proportions which the balls and tubes bear to one another; but, being marked on each inſtrument, theſe two points will enable us to perceive when the quickſilver is at any time reduced to either of theſe two determined ſtates of expanſion. And, when we have ſecured theſe two correſponding points in every thermometer, we can make ſure of a third, provided the tube be exactly cylindrical, or equally wide from end to end. We may take the middle point between the two above mentioned; it will ſhew when the fluid is reduced to a middling ſtate of expanſion between the two former; and, in this manner, we can proceed to ſubdivide the diſtance between thoſe two points into any number of parts or degrees, taking care only to divide it into the ſame number of parts or degrees in every thermometer, and to mark or number theſe degrees in the ſame manner in each. The correſponding degrees in the ſeveral thermometers will ſhew the correſponding ſtates of expanſion in theſe different inſtruments. And, if we deſire to meaſure or mark other ſtates of expanſion, above or below thoſe above mentioned, we can protract the ſcale above or below the primary points, by adding to it as many degrees of the ſame ſize as the tube will hold, and marking or numbering theſe alſo in a ſimilar manner in every thermometer. Or, we may chooſe other fixed points for adjuſting theſe parts of the ſcale, other ways having been found, by which the fluid of the thermometer can be reduced to certain determined ſtates of expanſion, different from the above; as, by immerſing it in boiling quickſilver, or into melting lead or tin, and ſome other metals, by which the quickſilver is greatly expanded; or, by putting it into a mixture of ſnow, with certain ſalts, by which it ſuffers an extraordinary contraction.

This method for conſtructing the ſcales of thermometers, by reducing the fluid to certain determined ſtates of expanſion, has great ad-

vantages over every other attempted before. It is eaſily practiſed, and as eaſily with ſmall thermometers as with thoſe of a larger ſize, which could not be done by thoſe other methods. Mr. Boyle firſt pointed out ſome of the facts on which this method proceeds; and Sir Iſaac Newton put it in practice in the 1701; and afterwards Dr. Hales, in the 1727, conſtructed ſix thermometers for himſelf, and took this method to make them agree; after which, the artiſts who make thermometers practiſed it with more or leſs accuracy. Dr. Martin, in his eſſays on heat and thermometers, has diſplayed the advantage of it in the cleareſt light.

To inſure a perfect correſpondence between thermometers, even by this method, there are ſeveral circumſtances which muſt be attended to, beſides thoſe mentioned by Dr. Martin. See in Phil. Tranſ. for 1788, an excellent paper on this ſubject by Mr. Cavendiſh.

Having thus explained the general principles of the art of conſtructing thermometers, the next queſtion is, whether the degrees of their ſcales expreſs, or point out, equal differences of heat? It is plain, that the immediate purpoſe for which the ſcale is applied, is not to meaſure heat itſelf, but the expanſion produced by heat. The ſcale of a thermometer divides the increments and diminutions of bulk into a number of ſmall and equal parts, that we may ſee by how many of theſe parts the bulk of the fluid is increaſed at one time, or diminiſhed at another: But it remains to be conſidered, whether theſe equal increments, or diminutions of bulk, be produced by equal increments, or diminutions of heat. We can imagine the fact to be otherwiſe, and that in ſome parts of the ſcale, a greater addition of heat may be required to produce one degree of expanſion, than in other parts. If a ſtring be ſtretched, by ſuſpending a moderate weight to it, and we add one pound to that weight, we ſhall make it a little longer; but, by adding a ſecond pound, we ſhall not add as much more to the length of the ſtring as the firſt pound added; nor will a third pound produce ſo much effect as the ſecond pound. In like

manner, we can imagine, that when a thermometer receives a ſeries of equal additions to its heat, theſe may not produce equal increments of expanſion; and, therefore, that equal increments of expanſion may require for their production increments of heat very unequal among themſelves. This queſtion has been overlooked, or little attended to, by ſome of the principal writers on thermometers. It does not appear to have occurred to Dr. Boerhaave; and Dr. Martin gives very little attention to it. I began to attend to it, and made an experiment to decide it in the year 1760, and did not then know that others had thought of it; but I ſoon learned that Boyle *, Renaldini of Padua, Wolfius †, Dr. Halley, Sir Iſaac Newton, and Dr. Brook Taylor, had ſeverally given their opinions or doubts concerning this queſtion; and ſome of them have deſcribed experiments, by which they thought it was decided. The moſt ſimple and ſatisfactory experiment for this purpoſe, is one deſcribed by Dr. Brook Taylor, in the 32d volume of the Philoſophical Tranſactions, publiſhed in the 1723. The experiment which I made, and communicated to the ſociety at Glaſgow, was the ſame with this; and Renaldini's experiment or proceſs was of the ſame nature.

Dr. Taylor's experiment was made by mixing cold and hot water together, by which we can produce various heats, or temperatures of heat, the differences of which from one another we can be ſure of, independently of any thermometer. If, for example, we ſuddenly mix a pound of hot water with a pound of cold, the exceſs of heat which was in the hot water, compared with the cold, being now equally divided between it and the cold water, muſt produce an exceſs or ſtrength of heat in the mixture, juſt the half of that which exiſted immediately before in the hot water. And, by applying a thermometer to the hot water firſt, next to the cold, and laſtly to the mixture, we learn whether the quickſilver is expanded by different

* Boyle, Abr. i. 580. † Wolfii Elementa Matheſeos, t. i. p. 780.

heats, in proportion to the differences of theſe heats. This experiment has accordingly been made, firſt by Dr. Brook Taylor in the 1723, next by myſelf in the 1760, at which time I did not know (as I ſaid juſt now) that it had been made or thought of by any other perſon: And ſince that time it has been repeated with much care and attention, and varied in the contrivance of it, by Mr. De Luc *, and Dr. Crawford †. To perform it well, attention muſt be given to two principal particulars: Firſt, it is neceſſary that the tube of the thermometer be exactly equal in wideneſs from end to end. This is aſcertained, by introducing as much quickſilver into it, before the thermometer is made, as will fill one inch, or one and a half inch of it, and meaſuring exactly the length of this little column of quickſilver, and then making it ſlide gradually through every part of the tube, and meaſuring and marking down its length in all theſe different ſituations. It is evident, that thoſe parts of the tube muſt be of equal wideneſs, in which the little column of quickſilver is of the ſame length, and ſuch parts only ſhould be employed in making thermometers.

The ſecond object, of neceſſary attention in performing the above experiment, is to make allowance for the heating or cooling of the veſſel in which the mixture is made. If the warm water is poured into the cold, the cold veſſel in which the mixture is thus made, will take from the mixture ſome of its heat, and make it appear colder than it ſhould otherwiſe do. If, on the contrary, the cold water is poured into the hot, the hot veſſel will impart ſome of its exceſs of heat to the more temperate mixture, and thus raiſe its heat above the degree which ſhould be produced.

To avoid this deception, we muſt employ the two veſſels of the ſame materials, and of the ſame ſize and weight, and then by making the experiment both the one way and the other, and taking the medium of the reſults, we ſhall learn the truth.

* De Luc on the Barometer. † Crawford on Heat.

The experiment being made with theſe precautions, the reſult has ſhewn, that when the thermometer is made of quickſilver, the gradual expanſions of this fluid, while it is heated ſlowly, from the cold of melting ſnow to the heat of boiling water, are very nearly proportional to the additions of heat by which they are produced. There is, however, a little deviation from the exact proportion; while the heat increaſes, the expanſions become a little greater than in proportion to the increaſe or addition of heat. The equable or uniform increaſe of heat, therefore, is attended with a ſmall acceleration of the increaſe of expanſion. This is much more remarkable in water, alcohol, and ſome other fluids, than in quickſilver. In this laſt fluid, this irregularity is ſo inconſiderable within the range of heat above mentioned, that it does not deſerve any notice in common experiments, and in the ordinary uſe of thermometers; but if it ſhould be neceſſary, on any occaſion, to eſtimate the amount of it in thermometers of quickſilver, and of ſpirit of wine, it may be collected from the experiments of Mr. De Luc, and of Dr. Crawford.

Here we may remark, that this little diſproportion between the degrees of expanſion and the degrees of heat, is not at all inconſiſtent with that ſuppoſition concerning the nature or cauſe of heat which I formerly mentioned as appearing to me the moſt probable; I mean the ſuppoſition that heat depends on the preſence or abundance of an inconceivably ſubtile ſelf-repelling matter, which is attracted by all other matter of the cohering or attracting kind. For the attraction of coheſion, that is, the attraction which the particles of a body have for one another, like all other attractions, is weakened by diſtance; and therefore when a body is expanded by heat, and the diſtance of its particles is a little increaſed, its attraction of coheſion being thus a little weakened, a further addition of heat to it can eaſily be imagined to produce more effect in expanding it further; the diſtending power of the repellent matter of heat being rather leſs counteracted by the attraction of coheſion, than it was at the firſt.

Such is the deciſion of this important queſtion concerning the degrees of thermometers. There is a paſſage, however, in Dr. Boerhaave's treatiſe on fire, which is inconſiſtent with what I have now repreſented to be the fact. He relates, that Fahrenheit had the curioſity to mix hot and cold water, in order to ſee what temperature they would produce. The reſult is given as matter of curioſity only. The importance of ſuch an experiment, as a way to verify the ſcale of heat, does not appear to have been thought of; and his account of the reſult is ſo contrary to the fact, and ſo abſurd in itſelf, that it is plain he had miſunderſtood Fahrenheit's account of what happened. He ſays, that whatever heat is in the colder water, that heat, and an equal quantity of the heat of the hot water, is extinguiſhed, and diſappears, and that the exceſs only of what is contained in the hot above the cold, remains in the mixture, and is equally diſtributed through it. If this were true, it would be eaſy, by adding hot water to cold, to produce a mixture colder than the cold water. Boiling water, added to water a little leſs hot, would produce a mixture colder than ice. Dr. Boerhaave's account of what happens in this experiment muſt therefore be diſregarded. We have clear and diſtinct accounts from others of what is the real fact, and I have often made the experiment myſelf.

Thus an important point is gained in our knowledge of heat, and in the uſefulneſs of thermometers. And, as it is proper, in order to underſtand the inſtruments thoroughly, to know the imperfections that ſtill attend them, I ſhall mention what may be conſidered as the remaining imperfections of our thermometers.

In the *firſt* place, they are always more or leſs ſlow in ſhewing the heat of other bodies to which they are applied. To underſtand how this happens, we muſt remember that the thermometer indicates immediately and directly that heat only which is in itſelf, but from the heat of the thermometer we judge of that of other bodies to which it is applied. This depends on the irreſiſtible propenſity of heat, which

I formerly mentioned, to be always communicated from any hotter body to the colder around, until they come to an equal temperature with one another. If the body we wiſh to examine be warmer or colder than the thermometer, no ſooner are they applied to one another than the heat begins to be communicated from the one to the other, until both are reduced to an equal temperature; and then, by examining the heat of the thermometer, we judge of that of the other body. As heat, however, is not ſuddenly but gradually communicated in this manner, we muſt wait ſome time after the application of the thermometer, before we can form this judgment with certainty. The way to diminiſh this inconvenience, as much as poſſible, is to make our thermometers very ſimple, and of a ſmall ſize, it being well known that ſmall maſſes of matter are heated and cooled much faſter than larger ones *.

A *ſecond* imperfection attending thermometers is, that the extent of the variations of heat, in the meaſurement or obſervation of which they can be employed, is limited within very moderate bounds. One reaſon of this is, that in ſtrong or violent heats, the increments of expanſion become more diſproportioned to the increments of heat. But, ſetting this aſide, it is well known that the fluid of the thermometer, if it were expoſed to a heat too ſtrong, would be converted into vapour highly elaſtic, which would burſt the glaſs; or if, on the contrary, it were expoſed to violent cold, the fluid would congeal. When we attend to theſe particulars, we find ſtrong reaſons for preferring quickſilver to all other fluids, for the conſtruction of thermometers; for beſide the advantages attending it, already taken notice of, it bears a much ſtronger heat than other fluids, without aſſuming the form of vapour, and it alſo endures very violent cold before it

* Beſides the other qualities of mercury, which make it fitter for a thermometer, it greatly excels all other fluids in the quickneſs with which it attains the temperature of the bodies to which it is applied.

EDITOR.

congeals. Lintſeed oil has alſo theſe qualities in a conſiderable degree, but the viſcidity of it renders it unfit for the conſtruction of ſmall thermometers. Spirit of wine is ſtill employed for ſome thermometers, on account of its great expanſion and contraction, it being almoſt eight times greater than that of quickſilver. It is the fitteſt fluid for the meaſurement of the moſt intenſe colds; for, when very pure, it has never been known to congeal; and it may alſo ſerve very well to ſhew the variations of heat that happen in the atmoſphere. But it is unfit for being employed, when we examine the heats that approach to that of boiling water, and thoſe above it. In ſuch heats, the expanſions of it become firſt exceſſive, and afterwards the ſpirit produces vapour, highly elaſtic, which burſts the thermometer.

The immediate application and uſe of thermometers is, therefore, limited to the meaſurement of a certain ſmall extent only of the variations of heat.

But, if we deſire to compare with theſe degrees, to which the thermometer can be applied, other more intenſe heats that are far above the reach of the inſtrument, this can be done by a method, of which an example is given in an ingenious paper on the degrees of heat, by Sir Iſaac Newton, in the Philoſophical Tranſactions for the year 1701. He took a lump of red hot iron out of the fire, and ſuddenly expoſed it to a ſtream of cold air, noting exactly the beginning of the time when it was expoſed. As ſoon as its heat was ſomewhat abated, he obſerved with attention the progreſs of its cooling, or how much heat it loſt in equal portions of time. This he did by the uſe of an oil thermometer, and alſo by laying upon the maſs of iron little bits of different metals, and of mixtures of metals, which he knew by previous experiments required certain intenſities of heat to melt them; and he noted the times when the heat of the iron was ſo much diminiſhed that it ceaſed to preſerve theſe little maſſes melted. Thus, having learned what heat was in the iron at different periods of the whole

time it required to cool it, he diſcovered the celerity and progreſſion with which it loſt its heat, and conſequently could calculate how much it had loſt from the beginning of expoſition, or how much its heat at that time exceeded the inferior heats through which it afterwards paſſed.

By ſuch calculations, therefore, we can form an eſtimate of the intenſity of violent heats to which thermometers cannot be expoſed, and thus remedy, in ſome meaſure, that defect of the inſtrument, by which its power is limited in the meaſurement of heat. I ſhall hereafter have occaſion to mention other methods by which the intenſity of violent heats may be meaſured, or eſtimated. At preſent I ſhall further notice, that this method of Sir Iſaac Newton's is uſeful on many other occaſions, beſide the meaſurement of violent heats. It enables us to uſe the thermometer for meaſuring tranſitory heats, notwithſtanding the ſlowneſs of it in pointing out the heat of other bodies. There are many experiments in which the heat we deſire to meaſure exiſts only for a moment at its higheſt degree. When hot and cold water, for example, are mixed together, a middle temperature is inſtantly produced; but the middle temperature continues but for a moment; the exceſs of heat, which the mixture contains above the heat of the ſurrounding bodies, begins immediately to be communicated to theſe, and the intenſity of it therefore, is very ſoon diminiſhed,—ſo ſoon, that there is not time for the thermometer applied to be raiſed to that intenſity, and therefore to indicate it. But the remedy is eaſy: We muſt note with exactneſs the moment of time at which we know the heat muſt be at the higheſt degree, and immediately after, or at the ſame time, apply the thermometer. The quickſilver will riſe, but after a little while will ceaſe to riſe, being now heated to the ſame degree with the ſurrounding mixture, the heat of which was abating ſlowly, while that of the thermometer rapidly increaſed. After the thermometer ceaſes to riſe, we muſt continue our obſervations. It will now fall very gradually and ſlowly, accompany-

ing, in its progreſs of cooling, the mixture, or matter, to which it is applied, and will therefore indicate the celerity and progreſſion with which ſuch mixture or matter loſes heat. But when we have learned this celerity and progreſſion with which the mixture cools, it will be eaſy to calculate how much heat it loſt from the beginning of the experiment, and, therefore, to eſtimate what was the intenſity of its greateſt heat. Without this method, invented by Sir Iſaac Newton, it would be impoſſible, in many caſes, to learn exactly the full ſtrength of theſe tranſient heats; the thermometer, on account of its ſlowneſs, never would indicate their full amount.

3dly, It may be conſidered as a third defect of thermometers, that they do not inform us of the proportions which different heats bear to one another. We cannot ſay that one heat is double, or triple of another in point of ſtrength or intenſity. They inform us only of the differences of heats, or the exceſs by which they ſurpaſs one another, or any given degree of heat. The reaſon of this is, that we are ignorant of the loweſt poſſible degree, or beginning of heat. Some ingenious attempts have been made to eſtimate what it may be, but they have not proved ſatisfactory. Our knowledge of the degrees of heat may be compared to that we ſhould have of a chain, the two ends of which were hidden from us, and the middle only expoſed to our view. We might put diſtinct marks on ſome of the links, and number the reſt, according as they are nearer to, or farther removed from, theſe principal links; but, not knowing the diſtance of any link from the end of the chain, we could not compare them together, with reſpect to this diſtance, or ſay, that one link was twice as far from the end of the chain as another.

The numbers, however, which have been affixed to the degrees of the ſcales are apt to occaſion miſtakes on theſe ſubjects; but theſe numbers muſt be conſidered as expreſſive only of the difference, or diſtance, of the ſeveral degrees from ſome well known degree, ſuch as

that of melting ſnow, or of boiling water. They have no relation to the loweſt poſſible degree, or beginning of heat.

As the ſcale called Fahrenheit's ſcale, is the one commonly uſed in the Britiſh dominions, it may be firſt deſcribed. The author of it did not chooſe to begin his ſcale at the cold of melting ſnow, as many others have done. He knew there were more intenſe colds, and he choſe to begin at the moſt intenſe cold known at that time, and which could eaſily be produced by art. This degree of cold is produced by mixing together ſnow and ſal ammoniac, or common ſalt. He covered his thermometer with a mixture of this kind, and marked the part of the tube to which the quickſilver deſcended. He next put the thermometer among pure melting ſnow, and marked alſo the part of the tube at which the quickſilver became ſtationary in this ſituation. Then, dividing the diſtance between theſe two points into thirty-two equal parts, he made theſe the degrees of his ſcale, and marked them with numbers, beginning at the bottom, and increaſing upwards: and he further protracted the ſcale with degrees of the ſame ſize, to the top of the tube, marking theſe with numbers increaſing arithmetically upwards, as far as the ſcale went. A thermometer, therefore, conſtructed in this manner, when put into pure melting ſnow, does not point to the beginning of the ſcale, but to the 32d degree. When expoſed to the air of this climate in the moſt temperate weather of ſpring or autumn, it points to the 48th or 50th degree. In our moderately warm ſummer air it riſes to the 64th. In the warm weather of very hot climates it riſes to 80 or 90, or ſometimes to 100 degrees. And it points to the laſt, or near to it, when applied to the warmeſt parts of the human body, or to blood flowing immediately from a vein or an artery: and when it is dipped into boiling water, it riſes to the degree 212.

But after this manner of graduating them had been uſed for ſome time, it was diſcovered that there were colds far more intenſe than that produced by ſnow and common ſalt, and that it was neceſſary for mea-

ſuring theſe, to make an addition to the lower end of the ſcale. This was accordingly done, in ſome of thoſe thermometers, by adding a ſeries of degrees of the ſame ſize with the former; but theſe added degrees were marked with a ſeries of numbers increaſing downwards, and therefore expreſſing increments of cold, and diſtinguiſhed from the degrees of the other parts of the ſcale, by being called the degrees below o in the thermometers that are made to contain them.

Theſe are the reaſons for the particular contrivance of Fahrenheit's ſcale *.

Since it came into uſe, however, the makers of thermometers, although they apply this ſcale, do not find the point at which the ſcale begins, by making a mixture of ſnow and ſalt. They find the points of pure melting ſnow, and of boiling water, and divide the interval into as many degrees as are contained between theſe two points in Fahrenheit's ſcale, which are 180; and, continuing the ſcale down below the melting ſnow point, they begin the numeration 32 degrees below that point. Or, if the thermometer be intended for meaſuring no greater heats than thoſe of the weather, or the human body, the fixed points from which the ſcale is conſtructed are the melting-ſnow point, and the 96th or 100th of Fahrenheit's ſcale, which is found either by the inſtrument's being held in the maker's mouth for ſome time, or by exact compariſon with a ſtandard thermometer.

Such is the ſcale of the thermometers generally known and uſed in this country. Thoſe which have come into uſe in other parts of the world, are divided and numbered in different ways, in conſequence of this circumſtance, that the inſtrument, for ſome time after its firſt in-

* Wolfius gives a different account of Fahrenheit's ſcale. He choſe the temperature of ſalt and ſnow for the beginning of his ſcale, and that of boiling mercury for its termination, and he divided the interval into 600 equal parts. This made 32 the temperature of freezing water, and 212 its boiling temperature. He alſo found that each diviſion of this ſcale was $\frac{1}{11111}$ of the bulk of the mercury.

EDITOR.

vention, having been imperfect and ill underſtood, every perſon who choſe to think, and to make experiments on the ſubject of heat, began by conſtructing a thermometer, intended to be more intelligible and exact than thoſe ſold by the ordinary makers. They therefore applied to them various ſcales, divided, and numbered according to ſome particular method of their own contrivance; and ſome one of theſe improved thermometers came afterwards, in each particular country, to ſuperſede all the reſt, and to be eſtabliſhed as the one choſen for general uſe.

In France, and ſome other parts of the continent of Europe, they make uſe of Reaumur's thermometer, in which the interval between the cold of melting ſnow and the heat of boiling water, is divided into 80 degrees only, the numeration being begun at the melting-ſnow point, with a ſeries of numbers increaſing upwards, and another increaſing downwards *. In Ruſſia, De Liſle's thermometer is the ſtandard, in which the numeration begins at the point of boiling water, with a ſeries of numbers increaſing downwards, and expreſſing, therefore, degrees of contraction; and the interval between the above point and that of melting ſnow is divided into 150 degrees, which he ſuppoſed to be 10000th parts of the bulk of the quickſilver nearly. In Sweden, Celſius's is the one eſtabliſhed in common uſe, in which the interval between the melting-ſnow point and that of boiling water, is divided into 100 degrees, the numeration being begun at the melting-ſnow point, with a ſeries of numbers increaſing upwards.

But, notwithſtanding this variety, it is eaſy to find the degree of

* There is, however, a great uncertainty and confuſion in the different accounts given by Reaumur of the conſtruction of his thermometers; and the obſervations which were made by their means cannot be underſtood without much inveſtigation, in order to diſcover which of his conſtructions had been employed. For many years paſt a thermometer is called Reaumur's, if 0 be the point of freezing water, and 80 the boiling point—Yet this is very different from *all* of his thermometers.

EDITOR.

Fahrenheit's ſcale, which correſponds with any degree in the ſcales of theſe ſeveral thermometers. The two fixed points, of melting ſnow, and boiling water, being the regulating points in every one of theſe ſcales, each degree between thoſe two points has ſome point of Fahrenheit's ſcale which correſponds with it, and the place of which in that ſcale can eaſily be found by calculation, or by the uſe of a diagram, in which theſe ſeveral ſcales are drawn parallel to one another. Dr. Martin has given an example, containing many thermometers.

The ſcales of thermometers being now every where regulated by the principles above explained, the inſtrument is become very generally uſeful. An univerſal language is now eſtabliſhed concerning the degrees of heat, or it is very eaſy to tranſlate the language or meaning of one ſcale into that of another.

IMPROVEMENT OF OUR KNOWLEDGE OF HEAT BY MEANS OF THE THERMOMETER.

1*ſt*, *Extenſion of our Ideas of Heat.*

THE uninterrupted contraction of the thermometer by a gradual abſtraction of heat, gives us the ſtrongeſt indication of the latter being the cauſe of the former. This is frequently accompanied by appearances which are altogether unlike; yet we do not doubt of their being only different effects of one cauſe. We have no doubt of the freezing of tin being produced in the ſame way as the freezing of bees wax, nay, as the freezing of water; yet in this laſt, heat gives place to another ſenſation, altogether different, namely, the ſenſation of cold. In common life, we never ſpeak of a diminution of heat in this caſe, but of an increaſe of cold. The continued contraction of the thermometer, and the analogy between the freezing of tin, of bees wax, and of water, ſoon induce the belief that all are cauſed in the ſame way, by the abſtraction of heat. Thus are our ideas of the operations of heat very greatly enlarged. We now know that its influence is ſo extenſive, and that it is diffuſed in ſuch quantity through

nature, that no maſs of matter has ever yet been found ſo cold as to be totally deſtitute of heat; for ſpirits of wine have never yet been frozen, and we believe that it only requires the diminution of its heat to produce this effect. We are ſure that it ſtill retained a quantity of heat, and, poſſibly, a very great quantity. This is proved by covering up ſuch a cold maſs with a mixture of ſnow and certain ſalts, both of which had been previouſly made as cold as the maſs. This mixture becomes intenſely colder than the materials of it were immediately before, as appears by the application of a thermometer to it, and by the readineſs and quickneſs with which it will freeze many fluids that cannot be frozen by any other means. It therefore reduces the maſs involved in it to the ſame intenſe coldneſs, or extracts from this maſs a quantity of heat, which we thus learn it had contained before.

We muſt, therefore, conſider the coldeſt bodies ever obſerved in nature as ſtill retaining a great deal of heat, the amount of which we cannot define; the beginning, or loweſt poſſible degree of heat, being yet unknown to us. Indeed, when we recollect that heat is continually emanating from ſurrounding bodies, and that other bodies abſorb it ſo much the more greedily as they are colder, we cannot conceive a body altogether void of it, if expoſed in the neighbourhood of others.

But, to give a clearer view of this ſubject, it may be proper here to mention a few examples of diminutions of heat, or violent cold, that have been obſerved.

Dr. Boerhaave, in his elements of chemiſtry, deſcribes with admiration an experiment made by Fahrenheit. The moſt violent cold known at the time when this experiment was made, was that expreſſed by the beginning of Fahrenheit's ſcale, or 32 degrees colder than melting ſnow; it is as much colder than melting ſnow, as melting ſnow is colder than the air of our ſummer weather; and it may be at any time produced, by mixing ſnow or pounded ice with common ſalt. A natural cold, in which the thermometer had fallen to this

degree, had been obſerved in Iceland, and often in Germany, and ſome other countries of the continent of Europe. The ſame degree of cold has alſo ſometimes been obſerved in Britain; but it rarely happens, and never continues in this iſland, except for a few hours, perhaps, of an extremely cold night in a very hard winter. It therefore never produces all its effects; its duration is not ſufficient to let it act with the whole of its force. But, in thoſe places in which a cold of this degree of intenſity continues for ſome time, it produces very remarkable effects, ſuch as ſplitting timber, by freezing the juices and humidity that are in the pores of it; freezing beer, wine, vinegar, and all other fermented liquors. When ſtones, or metals, which have been expoſed to it, are touched with the tongue, or the ſofter parts of the human body, theſe are inſtantly frozen and mortified, and the principle of life in them is extinguiſhed. No animals can endure ſuch a cold, except thoſe which are provided by nature with very thick and warm furs; and man is under the neceſſity of wrapping himſelf in a thick covering of the furs of theſe animals. Few vegetables are able to withſtand its power; but they are protected from it by very deep ſnow, the fall of which always precedes the ſeaſon of this mortal cold.

Such is the power of this cold, in thoſe countries in which it prevails, during a conſiderable part of the winter ſeaſon.

But, in the experiment related by Dr. Boerhaave, Fahrenheit produced by art, a cold which was no leſs than 40 degrees lower; it was produced by the action of the ſtrong nitrous acid on broken ice, to which it was applied in a very ingenious manner, to make it produce its greateſt effect.

Dr. Boerhaave was aſtoniſhed at the effect of this experiment; but, had he lived a few years longer, he would have heard of degrees of cold obſerved in nature, which approached to this, which were equal to it, or even exceeded it.

The firſt example is in the hiſtory of a journey to the north end

of the Baltic, by ſome of the academicians of France, at the king's expence, in order to make obſervations and meaſurements, for deciding the queſtion concerning the figure of the earth. Theſe gentlemen wintered under the polar circle, where, for ſome days in the middle of winter, the ſun does not riſe above the horizon, but only approaches to it, and gives a twilight. In that climate, they found it neceſſary to uſe all poſſible precautions to ſecure themſelves from the dreadful cold which prevailed. They prevented, as much as poſſible, the entrance of the external air into their apartments; and if at any time they had occaſion to open a window or door, the humidity of their breath, confined in the air of the houſe, was condenſed and frozen into a ſhower of ſnow; their lungs, when they ventured to breath the cold air, felt as if they were torn aſunder; and they often heard the rending of the timber around them by the expanſive power of the froſt in its pores. In this terrible cold, their beſt thermometer fell to a degree equal to the 33d below the beginning of Fahrenheit's ſcale, or the 65th below the cold of melting ſnow; it is as much colder than the degree of melting ſnow, as this laſt is colder than the human body in a healthy ſtate.

But, in other places, and at different times, an intenſity of cold has been obſerved, in ſevere winters, ſtill greater than this, and fully equal to the cold produced by Fahrenheit's experiment. Such a degree of cold has been obſerved at the Britiſh ſettlements in Hudſon's Bay, and in different parts of the Ruſſian dominions, eſpecially thoſe that are ſituated far to the north, or diſtant from the ſea, and expoſed to winds blowing over an extent of northern continent, ſuch as Kamtſchatka, Siberia, Peterſburgh, and even Moſcow. In ſome of theſe places, degrees of cold have been recorded, as occurring naturally, or produced by art, which even appeared to exceed by far the 40th degree below 0; ſuch as the colds obſerved in Siberia by Baron Demidoff*, in which the mercurial thermometer has many times fallen

* Phil. Tranſ.

to the 120th degree below 0. The cold obſerved by Profeſſor Pallas at Kraſnojark, in which his mercurial thermometer fell to 46 degrees below 0, in a regular manner; but being then moved, fell ſuddenly, with a ſtart, to the 80th degree, at which it remained a whole day; and, in the courſe of this day, he congealed ſome pure quickſilver, by ſimply expoſing it in an open cup to the cold air (Phil. Tranſ.) Alſo the famous experiments made at Peterſburgh with ſnow and nitrous acid, in December 1760, by Profeſſor Braun, in which he alſo congealed quickſilver, and ſaw his mercurial thermometer fall, in ſome of the experiments, to 300, or 400, or 500, and once to 600 degrees below 0 of Fahrenheit's ſcale.

But, as ſoon as thoſe experiments were publiſhed, it appeared to me perfectly evident, and I always declared it as my opinion, that the cold produced by Profeſſor Braun, in his mixtures, and that which was obſerved on ſome of the other occaſions juſt now mentioned, was not near ſo violent as it appeared to be by the extraordinary deſcent of the quickſilver in the thermometer. From many phenomena deſcribed in thoſe experiments, I found reaſon to conclude, that quickſilver, while it congeals, ſuffers an exceſſive and irregular contraction, much greater than in proportion to the diminution of its heat; that it is the oppoſite to water in this reſpect; for water, when congealing, is conſiderably expanded, inſtead of contracting; its expanſion being a ſingular phenomenon, which does not appear in any other caſe of the diminution of its heat. But quickſilver, on the contrary, while it congeals, is very much contracted, incomparably more than in proportion to the diminution of its heat, as meaſured by the thermometer. This was evident, from many phenomena which appeared in theſe experiments of Profeſſor Braun's, a few of which I ſhall here point out.

1*mo*, In the firſt place, when the mercurial thermometer, which was plunged into the mixture of ſnow and nitrous acid, fell to thoſe very low degrees, the deſcent of it was not regular and gradual. It deſcend-

ed indeed ſlowly and regularly a conſiderable way, in the beginning of each experiment; but, after this, its deſcent was irregular, by a rapid motion, or ſudden ſtarts of 100 or more degrees at once; and this happened eſpecially when the thermometer was moved or diſturbed. Now, it is well known, that in many fluids, when they are ſufficiently cooled to make them congeal, the beginning of congelation is promoted by diſturbing or ſhaking the fluid matter. I therefore concluded, that in the above experiments, the quickſilver in the thermometer was firſt gradually cooled to a ſufficient degree, to prepare it for congealing; and that during this diminution of its heat, it contracted in a ſlow and gradual manner, and that the ſudden and violent ſtarts of contraction which afterwards followed, proceeded, not from the farther diminution of heat, but from more or leſs congelation, which actually happened; for the Profeſſor broke ſome of theſe thermometers, and found the quickſilver congealed within them.

2do, Secondly, Profeſſor Braun, in ſome of theſe experiments, congealing ſome quickſilver in a wide and ſhort tube, the upper end of which was left open, ſaw, that after the quickſilver began to congeal, the upper ſurface of it became remarkably hollow during the progreſs of the congelation. But the contraction of the quickſilver which occaſioned this hollowneſs could not proceed from a proportional diminution of heat; for it is well known, and ſhall be illuſtrated in the ſequel, that every fluid continues of the ſame heat, from the moment when it begins to congeal, to that in which its congelation is completed, which generally requires a conſiderable time. The maſs does not become colder, although ſurrounded with colder matter, until the congelation of it is at laſt completed. The above exceſſive contraction of the quickſilver, therefore, after it began to congeal, was a concomitant of its congelation alone, and not of its becoming colder.

3tio, Thirdly, To eſtabliſh further the idea and belief of this fact with regard to quickſilver, we may remark, that the ſame thing is ob-

ſervable in other metals, particularly tin and lead. If either of theſe metals be melted in a crucible, and removed from the fire to cool ſlowly, they will ſoon begin to congeal, and, during the progreſs of their congelation, a conſiderable hollow, or a number of hollows, will be formed in the upper ſurface of the metallic maſs, although, during the time while theſe hollows are formed, the congealing metal does not become colder; of which we may be ſatisfied by immerſing the bulb of a mercurial thermometer into the fluid metal, and keeping it there until the congelation is finiſhed. In theſe metals, therefore, as well as in quickſilver, congelation is attended with an exceſſive contraction.

The degree of cold neceſſary for the congelation of quickſilver was not aſcertained with preciſion, until later experiments were made in a more accurate and ſatisfactory manner, with mixtures ſimilar to thoſe which Profeſſor Braun had uſed in imitation of Fahrenheit: And theſe later experiments have ſhewn that the degree of cold produced in ſuch mixtures is not near ſo violent as he had ſuppoſed it.

The firſt ſatisfactory proof we had of this was in ſome experiments made in the Britiſh ſettlements in Hudſon's Bay, by Mr. Hutchins, one of the ſervants of the Hudſon's Bay Company. His firſt attempts were made in conſequence of inſtructions from the Royal Society; and, as they had not been ſucceſsful in aſcertaining the degree of cold at which quickſilver begins to congeal, I wrote a ſhort letter, at the deſire of a friend, giving general directions, in a few words, for the proper conduct of the experiments. In the former experiments, the mercurial thermometers had been plunged immediately into the cold mixture, as in Profeſſor Braun's trials at Peterſburgh, and when the cold became ſo great as to congeal the quickſilver of the thermometer, the exceſſive and irregular contraction, which it then underwent, made it quite unfit for meaſuring the cold. I therefore propoſed, that if mercurial thermometers were employed, they ſhould not be plunged immediately into the cold mix-

ture, but into a quantity of quickſilver, contained in a ſmall glaſs, or wide tube, which glaſs and quickſilver ſhould be ſet in the cold mixture. Thus, while the quickſilver in the wide tube was gradually congealing, the thermometer itſelf, placed in the centre of it, would indicate exactly the degree of cold of the quickſilver congealing around it; nor could the cold congeal the quickſilver of the thermometer, ſo long as any part of the external quickſilver remained uncongealed. A ſecond ſet of inſtructions were ſent the following winter from the Royal Society, very full, and very accurately drawn up, and new experiments were made, which were quite ſatisfactory.

And the reſult was, that the cold, which is ſufficient to congeal quickſilver, exceeds but very little the cold which had been formerly produced by Fahrenheit. It is the 40th or 41ſt degree below 0 of his ſcale *, as meaſured with a mercurial thermometer, and the 30th, as meaſured with a thermometer made of the pureſt ſpirit of wine, a fluid which does not congeal in any degree of cold that has yet been obſerved.

This reſult has been further confirmed by another ſet of experiments made at Peterſburgh, by Dr. Guthrie, to whom I likewiſe gave a few directions to aſſiſt him in the conduct of them †.

We have reaſon, therefore, to be perfectly ſatisfied that the degree of cold at which quickſilver begins to congeal, and which is produced by a mixture of ſnow and ſtrong nitrous acid properly made, is not more than the 40th or 41ſt degree below 0 of Fahrenheit's ſcale. But this degree is as much colder than melting ſnow, as melting ſnow is colder than the human blood in a violent fever.

Even this degree of cold, however, has been ſince ſurpaſſed by the ſucceſſor to Mr. Hutchins at Hudſon's Bay. He was ſupplied with proper materials, and very full inſtructions, by Mr. Cavendiſh, in conſequence of which he made a ſet of experiments, by mixing ſnow

* Phil. Tranſ. 1783. † Phil. Tranſ. 1786-7.

with vitriolic acid in different ſtates. He thus produced colds, ſome of which made the beſt ſpirit of wine thermometers deſcend to the 69th degree below o of Fahrenheit's ſcale.

Thermometers, therefore, by enabling us to meaſure and compare great and uncommon diminutions of heat, have aſſiſted us greatly to enlarge, as well as to correct, our ideas of heat, to perceive its extenſive influence, and the great quantity of it which is diffuſed through nature: For there are many reaſons for being aſſured, that the 69th degree below o is far from being the extremity of cold, or loweſt poſſible degree of heat. When you ſhall be acquainted with the effects of mixture, and with the nature of the materials employed in theſe mixtures, you will not find it difficult to form a judgment with reſpect to this matter.

2. *Of the Diſtribution of Heat.*

A ſecond improvement in our knowledge of heat, which has been attained by the uſe of thermometers, is the more diſtinct notion we have now than formerly, of the *Diſtribution* of heat among different bodies.

I remarked formerly, that, even without the help of thermometers, we can perceive a tendency of heat to diffuſe itſelf from any hotter body to the cooler around, until it be diſtributed among them, in ſuch a manner that none of them are diſpoſed to take any more heat from the reſt. The heat is thus brought into a ſtate of equilibrium. This equilibrium is ſomewhat curious. We find that when all mutual action is ended, a thermometer, applied to any one of the bodies, acquires the ſame degree of expanſion: Therefore the temperature of them all is the ſame, and the equilibrium is univerſal. No previous acquaintance with the peculiar relation of each to heat could have aſſured us of this, and we owe the diſcovery entirely to the thermometer. We muſt therefore adopt, as one of the moſt general laws of heat, that " all bodies communicating freely with each other,

" and expoſed to no inequality of external action, acquire the ſame " temperature, as indicated by a thermometer." All acquire the temperature of the ſurrounding medium.

By the uſe of theſe inſtruments we have learned, that if we take 1000, or more, different kinds of matter, ſuch as metals, ſtones, ſalts, woods, cork, feathers, wool, water and a variety of other fluids, although they be all at firſt of different heats, let them be placed together in the ſame room without a fire, and into which the ſun does not ſhine, the heat will be communicated from the hotter of theſe bodies to the colder, during ſome hours perhaps, or the courſe of a day, at the end of which time, if we apply a thermometer to them all in ſucceſſion, it will point preciſely to the ſame degree. The heat, therefore, diſtributes itſelf upon this occaſion, until none of theſe bodies has a greater demand or attraction for heat than every other of them has; in conſequence of which, when we apply a thermometer to them all in ſucceſſion, after the firſt to which it is applied has reduced the inſtrument to its own temperature, none of the reſt are diſpoſed to increaſe or diminiſh the quantity of heat which that firſt one left in it. This is what has been commonly called an equal heat, or the equality of heat among different bodies; I call it the *equilibrium of heat*. The nature of this equilibrium was not well underſtood, until I pointed out a method of inveſtigating it. Dr. Boerhaave imagined, that when it obtains, there is an equal quantity of heat in every equal meaſure of ſpace, however filled up with different bodies; and Profeſſor Muſchenbroeck expreſſes his opinion to the ſame purpoſe: " Eſt enim ignis " æqualiter per omnia, non admodum magna, diſtributus, ita ut in pede " cubico auri et aëris et plumarum, par ignis ſit quantitas *." The reaſon they give for this opinion is, that to whichever of thoſe bodies the thermometer be applied, it points to the ſame degree.

But this is taking a very haſty view of the ſubject. It is confound-

† Muſchenb. Phyſica.

ing the quantity of heat in different bodies with its general ſtrength or intenſity, though it is plain that theſe are two different things, and ſhould always be diſtinguiſhed, when we are thinking of the diſtribution of heat. If, for example, we have one pound of water in one veſſel, and two pounds in another, and theſe two quantities of water are equally hot, as examined by the thermometer, it is evident, that the two pounds muſt contain twice the *quantity* of heat that is contained in one pound. But, undoubtedly, we can ſuppoſe that a cubical inch of iron may contain more heat than a cubical inch of wood, heated to the ſame degree; and we cannot avoid being convinced of this by daily experience. Let the iron and wood, for example, be heated in a bread-oven, to the greateſt degree which the oven commonly gives, and let them then be taken up and held in the hands of the perſon who lifts them; it is certain that the iron will feel hotter, and will continue for a longer time to feel hot in the hand which holds it, than the wood will do; that is to ſay, the iron will communicate more heat to the hand, and will continue longer to communicate heat, before it aſſume the temperature of the hand, than the wood will do. Or, on the contrary, let the iron and wood be both covered with ſnow, until the ſnow has reduced them to its own temperature, and then let them be both taken up, and held in the hands; the iron, in this caſe, will feel colder than the wood, and will continue for a longer time to feel cold in the hand which holds it; that is, it will draw heat faſter, and will continue for a longer time to draw heat from the hand, before it aſſume the temperature of that hand. It is therefore plain, that the iron has a greater capacity and attraction for heat, or the matter of heat, than the wood has; and that, when both of them have been expoſed a due time to the ſame heating cauſe, and appear equally hot by the thermometer, it contains a greater quantity of heat, or of the matter of heat, than the wood does; and, therefore, in cooling them both down to the temperature of the hand, a greater quantity of heat comes out of the iron. Or, on the contrary,

if both are equally cold, as examined by the thermometer, and we then make them both warm to the ſame degree, the iron takes in much more heat than the wood.

It was formerly a common ſuppoſition, that the quantities of heat required to increaſe the heat of different bodies by the ſame number of degrees, were directly in proportion to the quantity of matter in each; and therefore, when the bodies were of equal ſize, the quantities of heat were in proportion to their denſity. But very ſoon after I began to think on this ſubject, (anno 1760) I perceived that this opinion was a miſtake, and that the quantities of heat which different kinds of matter muſt receive, to reduce them to an equilibrium with one another, or to raiſe their temperature by an equal number of degrees, are not in proportion to the quantity of matter in each, but in proportions widely different from this, and for which no general principle or reaſon can yet be aſſigned. It will be proper to conſult, on this ſubject, the Comment. de Rebus in Medicina Geſtis, vol. 21, and vol. 26, containing the valuable experiments of Jo. Carl. Wilcke, extracted from the Swediſh Tranſactions. Alſo experiments of Profeſſor Godoïin, in the Nova Acta Reg. Societ. Upſalenſis, tom. 5. This opinion was firſt ſuggeſted to me by an experiment deſcribed by Dr. Boerhaave (Boerhaave Elementa Chemiæ, exp. 20, cor. 11.) After relating the experiment which Fahrenheit made at his deſire, by mixing hot and cold water, he alſo tells us, that Fahrenheit agitated together quickſilver and water unequally heated. From the Doctor's account, it is quite plain, that quickſilver, though it has more than 13 times the denſity of water, produced leſs effect in heating or cooling water to which it was applied, than an equal meaſure of water would have produced. He ſays expreſsly, that the quickſilver, whether it was applied hot to cold water, or cold to hot water, never produced more effect in heating or cooling an equal meaſure of the water than would have been produced by water equally hot or cold with the quickſilver, and only two-thirds of its bulk. He adds, that it was ne-

ceſſary to take three meaſures of quickſilver to two of water, in order to produce the ſame middle temperature that is produced by mixing equal meaſures of hot and cold water.

To make this plainer by an example in numbers, let us ſuppoſe the water to be at the 100th degree of heat, and that an equal meaſure of warm quickſilver at the 150th degree, is ſuddenly mixed and agitated with it. We know that the middle temperature between 100 and 150 is 125, and we know that this middle temperature would be produced by mixing the cold water at 100 with an equal meaſure of warm water at 150; the heat of the warm water being lowered by 25 degrees, while that of the cold is raiſed juſt as much. But when warm quickſilver is uſed in place of warm water, the temperature of the mixture turns out 120 degrees only, inſtead of 125. The quickſilver, therefore, is become leſs warm by 30 degrees, while the water has become warmer by 20 degrees only; and yet the quantity of heat which the water has gained is the very ſame quantity which the quickſilver has loſt. This ſhews that the ſame quantity of the matter of heat has more effect in heating quickſilver than in heating an equal meaſure of water, and therefore that a ſmaller *quantity* of it is ſufficient for increaſing the ſenſible heat of quickſilver by the ſame number of degrees. The ſame thing appears, whatever way we vary the experiment; for, if the water is the warmer maſs, and quickſilver the leſs warm one, by the above difference, the temperature produced is 130. The water, in this caſe, is become leſs warm by 20 degrees, while the heat it has loſt, being given to the quickſilver, has made this warmer by 30 degrees. And laſtly, if we take three meaſures of quickſilver to two of water, it is no matter which of them be the hotter. The temperature produced is always the middle temperature between the two, or 125 degrees, in the temperatures already mentioned. Here it is manifeſt that the ſame quantity of the matter of heat which makes *two* meaſures of water warmer by 25 degrees, is ſufficient for making *three* meaſures of quickſilver warmer by the ſame number of degrees.

Quickſilver, therefore, has leſs *capacity* for the matter of heat than water (if I may be allowed to uſe this expreſſion) has; it requires a ſmaller quantity of it to raiſe its temperature by the ſame number of degrees.

The inference which Dr. Boerhaave drew from this experiment is very ſurpriſing. Obſerving that heat is not diſtributed among different bodies in proportion to the quantity of matter in each, he concludes that it is diſtributed in proportion to the ſpace occupied by each body; a concluſion contradicted by this very experiment. Yet Muſchenbroeck has followed him in this opinion.

As ſoon as I underſtood this experiment in the manner I have now explained it, I found a remarkable agreement between it and ſome experiments made by Dr. Martin (Eſſay on the Heating and Cooling of Bodies) which appeared at firſt very ſurpriſing and unaccountable; but, being compared with this one, may be explained by the ſame principle. Dr. Martin placed before a good fire, and at an equal diſtance from it, a quantity of water, and an equal bulk or meaſure of quickſilver, each of them contained in equal and ſimilar glaſs veſſels, and each having a delicate thermometer immerſed into it. He then carefully obſerved the progreſs, or celerity, with which each of theſe fluids was heated by the fire, and raiſed the thermometers. He found, by repeated experiments, that the quickſilver was warmed by the fire much faſter than the water, almoſt twice as faſt; and after each experiment, having heated theſe two fluids to the ſame degree, he placed them in a ſtream of cold air, and found that the quickſilver was always cooled much faſter than the water. Before theſe experiments were made, it was ſuppoſed that the quickſilver would require to heat or cool it a longer time than an equal bulk of water, in the proportion of 13 or 14 to one.

But, from the view I have given of Fahrenheit's, or Boerhaave's experiment with quickſilver and water, the above of Dr. Martin's is eaſily explained. We need only to ſuppoſe that the matter of heat, communicated by the fire, was communicated equally to the quickſil-

ver and to the water, but that, as leſs of it was required for heating the quickſilver, than for heating the water, the quickſilver neceſſarily was warmed faſteſt of the two. And when both, being equally heated, were expoſed to the cold air to cool, the air at firſt took their heat from them equally faſt, but the quickſilver, by loſing the ſame quantity of the matter of heat that the water loſt, was neceſſarily cooled to a greater degree; it therefore became cold much faſter than the water. Theſe experiments of Dr. Martin, therefore, agreeing ſo well with Fahrenheit's experiment, plainly ſhew that quickſilver, notwithſtanding its great denſity and weight, requires leſs heat to heat it, than that which is neceſſary to heat, by the ſame number of degrees, an equal meaſure of equally cold water. The quickſilver, therefore, may be ſaid to have leſs capacity for the matter of heat. And we are thus taught, that, in caſes in which we may have occaſion to inveſtigate the capacity of different bodies for heat, we can learn it only by making experiments. Some have accordingly been made, both by myſelf and others. Dr. Crawford has made a great number of very curious ones, and his Theory of the Heat of Animals is founded partly on ſome experiments made in this manner, the reſult of which is given in his book on that ſubject.

It appears, therefore, from the general reſult of ſuch experiments, that if we had a thouſand maſſes of matter, of the ſame ſize and form, but of different materials, and were to place them all in the ſame room, until they aſſumed the ſame temperature; were we then to introduce into that room a great maſs of red hot iron, the heat of which, when communicated with all theſe different bodies at the ſame time, might be ſufficient for raiſing the temperature of them all, by 20 degrees; the heat thus communicated from the iron, although it produced an equal effect on each of theſe bodies, in raiſing its temperature by 20 degrees, would not however be equally divided or diſtributed among them. Some of them would attract and retain a much greater quantity of this heat, or matter of heat, than others; and the quantity received

by each would not be in proportion to their denſities, but in proportions totally unconnected with it; and perhaps not any two of them would receive preciſely the ſame quantity, but each, according to its particular capacity, or its particular force of attraction for this matter, would attract and require its own peculiar quantity to raiſe its temperature by the 20 degrees, or to reduce it to an equilibrium or equality of ſaturation with the ſurrounding bodies. We muſt, therefore, conclude that different bodies, although they be of the ſame ſize, or even of the ſame weight, when they are reduced to the ſame temperature or degree of heat, whatever that be, may contain very different quantities of the matter of heat; which different quantities are neceſſary to bring them to this level, or equilibrium, with one another.

It may be here remarked, that the diſcoveries which have been made in this way are very unfavourable to one of the opinions which have been formed of the nature of heat. Many have ſuppoſed that heat is a tremulous, or other, motion of the particles of matter, which tremulous motion they imagined to be communicated from one body to another. But, if this were true, we muſt admit that the communication would be in conformity with our general experience of the communication of tremulous motion. We are not at liberty to feign laws of motion different from thoſe already admitted, otherwiſe we can make any ſuppoſition account for any phenomenon that we pleaſe. The denſer ſubſtances ought ſurely to be moſt powerful in communicating heat to others, or exciting it in them The fact, however, in a great many examples, and yet not in all, is juſt the reverſe. Such an opinion is therefore totally inconſiſtent with the phenomena. I do not ſee how this objection can be evaded.

When a ſufficient number of experiments ſhall have been made to aſcertain with exactneſs the capacity of different bodies for the matter of heat, we may try a new method, different from that of Sir Iſaac Newton, for eſtimating the violent degrees of heat, which are far above the

reach of the thermometer. Suppoſe, for example, that a lump of ſtone is taken red hot from a ſtrong fire, and plunged into water, and that, after a little while, when it and the water are reduced to the ſame temperature, the water is found warmer than before, by 10 degrees. Suppoſe alſo, that we have learned by other experiments, that, when heat is communicated from this ſtone to an equal bulk of water, for every one degree by which the water becomes warmer, the ſtone becomes leſs warm by two degrees. Then, if the ſtone and water in this experiment were equal in bulk, it is plain that, while the water was heated by 10 degrees, the ſtone muſt have been cooled 20 degrees; but if, inſtead of their being equal in bulk, the ſtone had only the fiftieth part of the bulk of the water, it muſt have been cooled 50 times as much, or was 1000 degrees hotter before it was plunged into the water than it is now, for otherwiſe it could not have communicated 10 degrees of heat to fifty times its bulk of water.

We have now ſufficiently explained what has been learned of the diſtribution of heat among different bodies, the knowledge of which ſubject, ſo far as we have yet advanced in it, has been acquired by the uſe of thermometers. *(See Note* 1. *at the end of the volume.)*

3. *Of the Celerity of the Communication of Heat.*

A third improvement which has been made in our knowledge of heat by the uſe of thermometers, is in the inveſtigation of the celerity with which it is communicated from hotter bodies to colder ones, in different caſes; and of the progreſſion by which its tranſmiſſion is regulated on thoſe occaſions. A number of experiments relative to theſe queſtions are collected and compared in ſome of Dr. Martin's eſſays; and, in my opinion, a general account of what may be inferred from them may be given in three propoſitions, expreſſing general laws of the communication of heat, and by explaining a ſort of deceptions which occur in making the experiments.

The firſt of theſe propoſitions is, that for any thing we yet know to

the contrary, heat, or the matter of heat, is diſpoſed to enter into, or to leave the different kinds of matter with nearly equal celerity; or, that among the different kinds of matter, there is no difference, ſo far as we know, in the celerity with which the matter of heat is diſpoſed to enter into them, or leave them.

When the experiments which have been made to inquire into this point are examined, they ſeem at firſt to lead to a very different concluſion. Some kinds of matter, in the ſame circumſtances, are found to be heated and cooled much faſter than others. The firſt opinion formed upon this ſubject was, that the times neceſſary for the heating or cooling of equal maſſes of different matter are directly in proportion to the denſity of the maſſes; or that the celerity with which bodies of equal ſize are heated or cooled, is inverſely as their denſity, or directly as their rarity. Lord Verulam firſt entertained this opinion, or rather offered it as a ſuſpicion: But Dr. Boerhaave afterwards adopted it, and was ſo much perſuaded of it, that he gives a liſt or a number of ſubſtances differing greatly in denſity, ſuch as gold, quickſilver, water, oil, ſpirit of wine, æther, air, &c. and he affirms, that if equal bulks of theſe different bodies be reduced to the ſame temperature, and then expoſed to the action of the ſame heating cauſe, capable of making them hotter by 100 degrees, for example, each will require a different length of time from the reſt to receive that temperature; the denſer bodies requiring a longer time, in proportion to their denſity.

But this opinion is entirely the work of imagination, without any foundation in fact; and, when we have recourſe to experiments, the reſult proves it to be widely erroneous. This appears from ſome experiments of Dr. Martin's, one of which is the experiment on the heating and cooling of quickſilver and water, already deſcribed: in which the quickſilver, inſtead of requiring a much longer time than water to its being heated or cooled, was heated or cooled in the ſame circumſtances, much faſter than the water.

I am perſuaded that when different kinds of matter are equally expoſed to the ſame heating cauſe, the matter of heat enters into them all at firſt with nearly equal celerity; at leaſt we have no facts to prove the contrary. But there is neceſſarily a deception in the appearances, ſome of them becoming hot ſooner, and ſome much ſlower than others, and for this reaſon, that thoſe which have the greateſt capacity for the matter of heat, are not ſo ſoon ſaturated with it as others. They require a greater quantity of it, and a longer time for that quantity to be communicated to them; and I apprehend that the time neceſſary for the heating or cooling of different bodies of the ſame ſize, and in the ſame circumſtances and conditions, will be found nearly proportional to the capacity which each body has for the matter of heat, or to the quantity of it neceſſary to raiſe its temperature by a given number of degrees; and that, in Dr. Martin's experiment with quickſilver and water, although the quickſilver became hot, and appeared to take in the heat faſter than the water, it did not in reality take it in faſter, but, having leſs capacity for heat, was ſooner ſaturated to that degree which accorded with the circumſtances in which it was placed.

For any thing, therefore, that we yet know to the contrary, there is no difference among the different kinds of matter, with reſpect to the celerity with which heat is diſpoſed to enter into them, or leave them, when the circumſtances are exactly the ſame. (*See Note* 2. *at the end of the volume.*)

The ſecond propoſition, or general law, which may be deduced from the experiments on the heating and cooling of bodies, is that the celerity with which heat is communicated from hotter bodies to colder ones, is, when other conditions are equal, nearly proportional to the difference of their temperature, or to the intenſity of the heat in the hot body above that of the cold.

Of this general fact we have examples in the experiments that have been made to learn the progreſs with which bodies cool, when expoſ-

ed to a ſtream of cold air. The heat is taken away faſteſt at firſt, and more and more ſlowly afterwards, in proportion as the temperature of the maſs approaches nearly to that of the air which is moving againſt it.

Sir Iſaac Newton ſuppoſed, that, in this caſe, the quantities of heat loſt, in equal and ſmall portions of the time, are exactly proportional to the exceſs of heat remaining in the hot body at the beginning of ſuch ſmall portions, or to the difference between its temperature and that of the air; and, therefore, that the quantities of heat loſt in equal diviſions of time are a ſeries of proportionals, or in a geometrical progreſſion *. This idea of Sir Iſaac Newton's is ſupported or verified by ſome experiments which were made, in a great variety of circumſtances, by the ingenious Profeſſor Richman, and publiſhed in the tranſactions of the Peterſburgh academy for the year 1750.

Dr. Martin, it is true, alſo made ſome experiments relative to this queſtion, by which experiments he thought it appeared that the progreſſion of cooling varied a little from this rule †. But I am not ſatisfied with Dr. Martin's experiments on this ſubject. I think they were attended with a circumſtance that might deceive him. There is room, however, for more inquiry into this point by further experiments.

It is proper, alſo, to remark, that experiments made with this view can have no value, except when the heat is taken from the maſs, or communicated to it, by a ſtream of air playing conſtantly and uniformly on it, and all the time at the ſame temperature; for in this caſe only can it act in a regular manner. If, on the contrary, the air be not in motion, but in a ſtagnating ſtate; and ſtill more, if the hot body be incloſed with a ſmall quantity of it, the natural progreſs of cooling muſt be totally deranged, as in this caſe, the cooling cauſe will not continue the ſame, the air becoming warmer every moment, while it cools the hot body, and therefore leſs fit to take away the reſt

* Phil. Tranſ. abridged, IV. 2. p. 3.

† Martin's Eſſays, p. 55, &c.

of its heat. This enables us to underſtand why, in cold weather, if the air is calm, we are not ſo much chilled as when there is wind along with the cold; for in calm weather, our clothes, and the air entangled in them, receive heat from our bodies; this heat is accumulated in them to a certain degree, and brings them nearer than the ſurrounding air to the temperature of our ſkin. But in windy weather, this heat is prevented, in a great meaſure, from accumulating; the cold air, by its impulſe, and rapid ſucceſſion, both cooling our clothes faſter, and carrying away the warm air that was entangled in them.

The ſenſation of coldneſs, therefore, produced by wind, or agitated air, is ſo much ſtronger than that produced by equally cold air in a ſtagnating ſtate, that we are often perſuaded the agitated air is actually colder, until we examine it by the thermometer; and Dr. Boerhaave thought the deception ſo ſtrong, that he contrived an experiment to remove it completely (Boerhaave Elementa Chemiæ). He ſuſpended a thermometer in the air of a large room, for ſome time, and noting the degree to which it pointed, he then directed againſt the bulb of it a ſtream of air, impelled by a large bellows, in the ſame room. That ſtream of air would certainly feel to a perſon who oppoſed any part of his body to it, conſiderably colder than the reſt of the air in the ſame room; but the thermometer is not in the leaſt affected by it. And it would be eaſy to exhibit another experiment, to ſhew, that agitated air is not made colder by agitation. A piece of ice, for example, being ſuſpended in the air of a warm room, and blown upon by bellows, inſtead of being thereby kept the more cool, as our hand would be, and preſerved the longer from being totally melted, would certainly be melted ſo much the faſter, than when the air is allowed to ſtagnate in ſome meaſure around it.

The laſt of the three propoſitions which I ſaid may be deduced from the experiments on the communication of heat is, that

The celerity with which heat is communicated from hotter bodies to

colder ones, when all other conditions are equal, is proportionable to the extent of contact, and cloſeneſs of communication, between the bodies.

Every perſon is prepared to admit this general rule by common experience. We all underſtand that, if we had two balls, and two cubes of gold, and one of each was made very hot in an oven, and then laid upon the other, the heat would be communicated much faſter from the hot cube to the cold one, than from the hot ball to the cold one, which is touched by it in one point only.

Of the general fact expreſſed by this propoſition, we have examples in the experiments that have been made on the heating and cooling of maſſes of the ſame matter, but of different forms, and different ſizes, expoſed at the ſame time to be heated by a conſtant and equal heating cauſe, or to be cooled by a ſtream of cold air. It being at the ſurface that the body has contact and communication, either with the cold air or with the heating cauſe, the extent of contact and communication of parts is, in this caſe, the whole ſurface. Accordingly, thoſe forms which contain the ſame quantity of matter under the greater ſurface, are the more quickly heated or cooled, in proportion to the extent of that ſurface; an ounce of gold in the form of a cube, is heated and cooled ſooner than the ſame quantity in the form of a ball; and if it be beaten into the form of a thin plate, it is heated and cooled incomparably ſooner, in proportion as the ſurface is then the more extended; and, for the ſame reaſon, when the maſſes we compare have the ſame form, and differ only in their ſize, the ſmaller are heated or cooled faſter than the larger, or in the ſhorter time, in the direct proportion of their dimenſions. If we have, for example, two cubes of gold, in one of which the diameter is only the half of that of the other, and both of them are plunged into boiling water to be heated; or after they are heated, to be expoſed to a ſtream of cold air to cool, the ſmaller will be heated or cooled in half the time that is required by the other; for, in the ſmaller body, the ſurface bears twice the pro-

portion to the quantity of matter, and conſequently to the quantity of heat, which is to be either communicated or taken away, that it does in the larger, the quantity of matter being as the cube, and the ſurface only as the ſquare of the diameter. And, accordingly, ſome experiments made by Sir Iſaac Newton, and by Dr. Martin, agree ſufficiently with this general rule, which applies equally to all maſſes of unequal ſize, but of the ſame matter, and having the ſame form, whatever that be.

This enables us to underſtand how it happens, that in thoſe arts in which much heat and fuel are employed, as in the extraction of metals from their ores, founderies, diſtilleries, and the like, experience has ſhewn, that the larger they make their furnaces and ſtills, and other ſuch utenſils, they do their work with ſo much the leſs fuel; for, in the larger furnaces and ſtills, and other ſuch utenſils, the extent of ſurface, communicating with the air, bears a ſmaller proportion to the contents of the furnace or ſtill, and to the quantity of fuel employed, or of heat produced, than in ſmaller utenſils of the ſame nature; and the waſte of heat is therefore leſs.

We can alſo explain, by the ſame general principle, the ſlowneſs with which heat is communicated from one part to another of thoſe bodies, or aſſemblages of bodies, which are uncommonly rare or ſpongy, through which it paſſes much more ſlowly than through the denſe and heavy. A thick rod of iron, for example, or copper, if one end of it be put into the fire, and made red hot, tranſmits the heat ſo quickly, and in ſuch quantity, to the other end, that it cannot be touched without burning the hand; while a ſtick of wood of the ſame ſize can eaſily be held by the one end, while the other is burning. A piece of cork tranſmits heat through it ſtill more ſlowly than the wood, or is a worſe conductor of heat; and a maſs of wool, or feathers, or furs, excel even the cork in the ſlowneſs with which heat is conducted through them. Such materials compoſe a maſs ſo ſpongy and rare, and between the parts of which there is ſo little cloſeneſs of con-

tact, that the heat is communicated with difficulty through them ; and, although the interſtices or pores of ſuch bodies are filled with air, this being a fluid exceſſively rare, and which, in this caſe, is kept entangled, and is in a ſtagnating ſtate, it gives little aſſiſtance in the tranſmiſſion of heat. Theſe are the reaſons why ſuch materials are ſo effectual in keeping our bodies warm in very cold weather. In very cold countries, the inhabitants are not only wrapped in a thick clothing of furs by day, but cover themſelves with feather beds by night. Frequent experience of the effect of theſe materials diſpoſes us to conſider them as warm, or as having the power of adding heat to the bodies wrapped in them ; but this is a miſtake, they only preſerve the heat of ſuch bodies from being diſſipated ſo faſt as it would otherwiſe be ; and, by the ſame quality, they may be, and are, employed to keep things cool in ſome caſes. If, for example, we wiſh to preſerve a lump of ice from melting faſt in a warm room, we cannot do better than to wrap it in plenty of flannel, or furs, or the like materials, which produce the deſired effect, by retarding the communication of heat to the ice from the air, and other bodies contained in the room.

Theſe coverings, therefore, of wool, furs, feathers, and ſuch like materials, keep us warm, not by adding heat to our bodies, but by preſerving our natural heat from being diſſipated too faſt.

In the ſame manner does ſnow preſerve the ground from being deep frozen, and the vegetables from being deſtroyed, in the very cold countries. Nothing can be imagined more ſpongy, ſoft, and downy, than ſnow that has not been compreſſed ; nothing is more fit to ſerve this purpoſe. The ſhowers of it that fall in this climate, may be conſidered as accidental, and of little importance; the moderate temperature of our winters rarely requires ſuch a preſervative. But in thoſe parts of the world that are expoſed to violent cold, it is well known to fall regularly in the beginning of winter, until it cover the ground to the depth of ſeveral feet, and, where the cold is exceſſive, to that of ſe-

veral yards, and even of fathoms. Thus nature, which has clothed the animals of thoſe regions with the richeſt and warmeſt furs, throws over the ground this thick and downy covering, which has a wonderful effect in preſerving its heat. It cannot, it is true, preſerve it at a higher degree than the freezing point; for unleſs the ſurface be cooled to this degree, the ſnow cannot be on it. But this degree is a great degree of heat, when compared with the terrible and deadly cold which is common during the winter in the air of thoſe climates. In countries expoſed to ſevere winters, the thermometer commonly falls to the beginning of Fahrenheit's ſcale, or 32 of his degrees below froſt: a degree of cold which is juſt as much colder than ſimple froſt, as froſt is colder than the warmth of our ſummer weather. And, in the countries the moſt remarkable for ſevere winters, a cold has often been obſerved, which was more than twice as far below froſt. In Siberia, at Kraſnojark, eaſt of Siberia, and in ſome of our ſettlements in Hudſon's Bay, the thermometer has often fallen to 35 and 45 degrees below 0 of Fahrenheit: a degree of cold in which quickſilver was congealed by ſimply expoſing it to the cold air.

In ſuch dreadful colds, a great depth of ſnow lying on the ground has much effect in preſerving the vegetables.

It is proper, however, in this place to remark, that this effect of the rarity, or ſpongineſs of bodies, this retardation of communication, or tranſition of heat through them, does not take place in fluids equally as in ſolids. Fluids generally receive heat, and tranſmit it through them more quickly, than the greater number of ſolids. Of this we have an obvious example in air, which, though exceſſively rare and light, cools the bodies expoſed to it remarkably faſt; much faſter than we might expect from our knowledge of its rarity.

This depends, partly, on the capacity which ſuch fluids may have for the matter of heat, but more on their great expanſibility by heat, and on the eaſy mobility of their parts among one another; which two laſt qualities, combined in the ſame matter, produce a particular

effect, which occaſions the quick diſperſion of the heat through it. This effect is a conſtant motion, or change of the fluid in contact with the heated body.

If, for example, we expoſe a hot body, as a lump of red hot iron, to the air to cool, the air in contact with it becomes immediately hot, while it takes heat from the iron. In becoming hot it is alſo greatly expanded, and rendered more light than the ſurrounding parts of the atmoſphere. It is, therefore, preſſed upwards by theſe, in the ſame manner as a piece of cork, a quantity of oil, or any lighter ſubſtance, is preſſed upwards when we attempt to ſink it in water. It does not, therefore, remain in contact with the iron, but is forced to riſe in a ſtream, and leave its place to colder air, which comes from all around. This again is ſoon heated and expanded, and made to riſe in the ſame manner, and thus freſh and cool air is applied to the hot iron in conſtant ſucceſſion, by which its heat is carried off much faſter than if the matter in contact with it were perfectly ſtagnant.

This aſcending motion of the air that is in contact with a hot body becomes evident when we hold our hand perpendicularly over it, though at a conſiderable diſtance, or when we place the hot body in the light of the ſun, and ſo that the ſhadow of it may be thrown on a white wall: a waving ſtream, like a ſtream of ſmoke riſing from the ſhadow, is ſeen on the wall. This appearance is cauſed by the rarefied air, which, having its power to refract or attract light conſiderably changed by its rarefaction, the light which paſſes through it is turned out of its courſe to one ſide or the other, in a fluctuating manner, and therefore does not fully and regularly illuminate that part of the wall to which it was tending, but produces an imperfect and unſteady illumination, which reſembles the ſhadow of ſmoke.

Another effect produced by this motion of warm air, is the tremulous appearance of objects ſeen at a diſtance through the ſmoke, or heated air, ariſing from a fire, or from chimney tops, or from dry ground much heated by the ſun; for the light which comes from

theſe objects to the eye, is diſturbed a little from its ſtraight courſe, by paſſing through the ſtreams of warm air; and coming, therefore, to the eye, with ſucceſſive changes of its direction, occaſions an unſteadineſs in the apparent place of the object, or parts of the object, from which it comes.

We alſo learn from this effect of heat upon air, the reaſon of the common opinion, that heat, or fire, has a tendency upwards. This opinion has been founded on the ſtream of warm air which may be felt aſcending from any hot body expoſed to the atmoſphere; and the appearance and properties of flame have alſo contributed to the ſupport of ſuch an opinion. Flame, in conſequence of its being compoſed of a luminous fluid, rarer than air, is driven upwards, in this laſt, on every occaſion. To be convinced, however, that heat has not a prevalent tendency upwards, we need only ſhut up a hot body in a glaſs receiver, and exhauſt the air: We ſhall then find the heat penetrating equally through every part of the receiver; or, if it affects ſome more than others, it is thoſe that are neareſt the hot body. But, let the air be admitted into the receiver, the uppermoſt part of the glaſs will ſoon be the hotteſt, although the moſt diſtant from the hot body.

Heat, therefore, by rarefying air, and thereby producing a motion in the parts of this fluid, is more quickly and equally diſperſed through it than through any ſolid quantity of matter, and occaſions the appearances above deſcribed.

There is one other fact that depends on this principle, and which deſerves particular notice, on account of its importance. This fact is the ventilation of mining works that are properly planned, and the proper effect of fire-places and vents in houſes rightly conſtructed.

In mines that are properly planned, a ſtream of air paſſes conſtantly through the mine, and with this remarkable circumſtance, that it has one direction in ſummer, and another in winter.

To underſtand this, it is neceſſary to know, that in beginning to

mine for metals or minerals, the mountain or riſing ground in which they are contained, is firſt pierced in an horizontal direction, that the water which abounds in ſubterraneous places may run out conſtantly, while the workmen proceed forwards to the vein of ore. This horizontal entrance into the mountain, or mining ground, is called the level, drain, or drift of the mine, or ſometimes the adit. But when they have proceeded forwards a certain length, they find themſelves diſtreſſed for want of freſh air. The long excavation they have made contains a quantity of air in a ſtagnating, or nearly ſtagnating ſtate, which is exhauſted of its vital principle by their breathing it, and by their candles. Their candles, therefore, firſt burn dim, and at laſt are extinguiſhed, and give them warning that their own lives are in imminent danger. The beſt remedy for this is, to dig a wide pit perpendicularly from the ſurface over the internal extremity of the level, ſo deep as to communicate with it. No ſooner is this done, than there is plenty of freſh air in the mine, a current is immediately eſtabliſhed, which in ſummer enters through the pit, and paſſes out through the level, and in winter as invariably enters through the level, and riſes up through the pit.

The reaſon of this is, that the heats of ſummer, and the colds of winter do not penetrate ſo quickly into the interior parts of the earth, as to produce there the ſame changes of temperature that happen in the atmoſphere. The communication of heat through large maſſes of ſolid matter being ſlow, there is not ſufficient time, during the ſummer, for the heat of the air, or that produced by the ſun's light, to act ſo completely on the maſs of this globe, or to give to the internal parts of it the ſummer heat of the atmoſphere; and, for the ſame reaſon, the winter is too ſhort to admit of ſuch a loſs of heat from the interior parts of the earth, as to bring down the heat of theſe to a level with that of the ſurface and the atmoſphere.

In the interior parts, therefore, the courſe of time has produced a temperature, between the heat of ſummer, and the cold of winter,

which undergoes very little variation. Of this we have experience when we place thermometers in deep caves, or pits, that have little communication with the atmoſpherical air, or when we plunge them into the water of perennial ſprings, where they riſe out of the ground. The thermometer points to the ſame degree in ſummer and in winter; which degree is the medium between the two ſeaſons in that particular part of the earth's ſurface. It is evident, therefore, that the internal parts of the earth, and along with them, the air contained in the cavities of mines, muſt be warmer in winter, and colder in ſummer than the external atmoſphere. Hence the column of air contained in the pit of the mine is heavier in ſummer, and lighter in winter, than an equal column of the external air; and this occaſions the motion which takes place,—for the internal column gravitating on the internal extremity of the level, is counterpoiſed by an external column which gravitates on the external extremity of the ſame level, and, whenever one exceeds the other in denſity and weight, the heavier muſt force the lighter one to aſcend. The internal column, therefore, deſcends in ſummer, and is forced upward in winter, its place being ſupplied by external air, which enters during the firſt ſeaſon at the top of the pit, and during the latter at the external extremity of the level. And this air, when it enters, being very ſoon reduced to the temperature of the internal parts of the earth, the ſame conditions continue to take place, and the current of air through the mine continues as long as the heat of the external air is different from the temperature of the internal parts of the earth. The truth of this explication is remarkably confirmed, by a ſtagnation of the air in ſuch mines, which now and then happens in the ſpring and autumn, the atmoſphere in thoſe ſeaſons ſometimes aſſuming the middle temperament. The cauſe of the motion is this diſcontinued, and the workmen muſt have, at ſuch times, recourſe to other means, to obtain a renewal of the air in the mine; ſuch as, kindling a fire at the bottom of the pit, to give a little ſuperiority of heat to the column of air which it contains, or directing

into it the force of the wind to ſet it in motion. But theſe expedients are employed only on particular occaſions. The ventilation of mines, in general, depends on the principle above explained. If it ſhould appear difficult to imagine how the external air ſhould thus paſs conſtantly through the mine, without reducing it to a degree of heat coinciding with its own, it muſt be conſidered that the ſolid maſs of matter, through which the mine is dug, is ſo enormouſly great, when compared with the ſmall ſtream of air which paſſes through the works, that this air cannot have any ſenſible effect in changing the temperature of the internal parts of the mine. They, no doubt, receive a little heat in this way during the ſummer, and loſe a ſmall part of their heat during the winter; but this acquiſition or loſs is too ſmall to prevent the ordinary conditions from continuing in force. It remains only to be further obſerved, with regard to this ſubject, that many mines are ſo extenſive, or of ſuch a nature, that it is neceſſary for drawing out the water or the rubbiſh, or other materials cut out of the mine, to dig ſeveral pits, at conſiderable diſtances from one another, to communicate with different parts of the ſubterraneous excavations. Theſe different pits, however, produce the ſame effect as the ſingle pit and level, above ſuppoſed; for being different from one another in depth, in conſequence of the unevenneſs of the earth's ſurface, the pits which are dug in the higher parts of the ground contain an higher column of air than thoſe dug in the lower; and, though the temperature of heat is the ſame in them all, and equally different from that of the atmoſphere, the difference of height produces a preponderancy in the one or the other; and the ſame effect is produced in ventilating the mine. The ſame principles explain the ventilation, or draught of air, as it is called, in common chimneys and vents,—and the vents of the lower rooms in a houſe carry up the ſmoke better than the vents of the upper rooms that are nearer the top of the houſe; but the venting of chimneys is liable to many accidents and variations, which are afterwards to be conſidered.

All theſe motions produced by heat in atmoſpherical air, and the quick diſperſion of it through it, depends on two qualities, formerly mentioned as belonging to fluids in general; I mean the eaſy mobility of their parts among one another, and their expanſibility by heat; and as theſe two qualities are found in air, in an eminent degree, it is, therefore, the more remarkably affected in the manner I have deſcribed.

All other fluids, however, are affected in a ſimilar manner, by heat applied to them; but not being ſo expanſible, the motion produced in them is not ſo conſpicuous. But a motion is produced in them, as well as in air, which occaſions the heat applied to be more quickly and equally diſperſed through them than through moſt ſolid bodies.

When water, for example, is ſet in a veſſel over the fire, the heat is neceſſarily communicated firſt to the lower parts of the fluid, and, while theſe receive the heat, they are expanded and become lighter than the reſt of the water; they are therefore driven upwards, and their place at the bottom is ſupplied by the colder, heavier parts of the fluid. Theſe, however, are ſoon heated, and made to riſe in their turn, and thus there is a conſtant motion, or circulation of the parts of the water, which occaſions a quick and equal diſtribution of the heat through it. For this reaſon, water is often employed in chemiſtry, as a medium through which heat is communicated to veſſels of glaſs, theſe being moſt ſecure from being broken when the heat is applied to them gradually and equally, over the whole of their ſurface. When water is applied to this purpoſe in chemiſtry, it is called the *Water Bath.*

A motion, depending on the ſame principle, is alſo the cauſe which hinders the water of deep lakes from being frozen during the winter, in this climate, and at times when the ſurface of pools, or other ſhallow collections of water, is totally frozen. The cold air, while it paſſes over the ſurface of the water, neceſſarily cools it. The ſuperficial water, by being thus cooled, becomes denſer and heavier than

the water that is below it. It therefore ſinks downwards, in innumerable ſmall ſtreams, leaving its place at the ſurface to be occupied by the leſs cold water which riſes from below. This, in its turn, is deprived of a part of its heat, and diſpoſed to ſink in the ſame manner; and thus a conſtant circulation of the parts of the water is eſtabliſhed, a conſtant exchange between the ſurface and the bottom; in conſequence of which there is ſuch a quantity of water to be cooled, when the lake is large and deep, that there is not ſufficient time, during our froſty ſeaſon, for its being all cooled to near the freezing point. And it muſt be cooled to within a few degrees of that point, before that part which is at the ſurface will remain there long enough to be frozen *.

But large ſeas, which contain collections of water incomparably greater than thoſe of lakes, and which are agitated by winds and currents, are, in conſequence of theſe circumſtances, ſo little affected by the cold of winter, that their temperature continues nearly the ſame through the whole year, like that of the interior parts of the earth. And this mild temperature of the ocean affects the air which paſſes over it, and the countries that are entirely or moſtly ſurrounded with it; when places ſurrounded with extent of continent, or expoſed to winds blowing over a northerly continent, are affected in winter with violent cold. As the heat of ſummer penetrates but ſlowly into the ſolid maſs of this globe, ſo, during the winter, it comes out again but ſlowly. The ſolid parts of the globe are at reſt among one another, and cannot be ſet in motion by moderate heat, like the parts of fluids. Heat, therefore, is tranſmitted through them with that ſlow progreſs with which it commonly pervades large maſſes of ſolid matter. The cold winds, therefore, while they blow over a great continent, very ſoon cool the ſurface of the earth. The heat of the interior parts of it does not quickly aſcend to temper that coldneſs, as it does from the

* When water is cooled to 6 or 8 degrees above freezing, it is not made ſenſibly denſer by a diminution of heat.—*De Luc.*

bottom of large collections of water. The ſurface, therefore, is cooled to a great degree, and the cold winds afterwards, while they paſs over this ſurface, not meeting with any thing that can diminiſh their coldneſs, act with their full force on the matter expoſed to them. *(See Note 3. at the end of the volume.)*

When we now reflect on thoſe motions which heat produces in fluids, the circulation of them which it occaſions, and the quick and equal diſtribution of heat through them, which follows from thence, we find one phenomenon in nature, which, on the firſt view of it, may appear inconſiſtent with theſe general principles. This phenomenon is the unequal diſtribution of heat in the atmoſphere, in which it is well known that the lower parts are much warmer than the ſuperior. We have ſatisfactory proofs of the coldneſs of the ſuperior parts of the atmoſphere, when we aſcend high mountains, and other elevated parts of the earth's ſurface, in whatever climate be their ſituation. The French academicians who were ſent to meaſure a degree of the meridian at the equator, when they left the ſultry plains of South America, and aſcended the high mountains of that part of the world, paſſed through a ſucceſſion of ſtrata of the atmoſphere, increaſing in coldneſs, and diſtinctly marked by their productions, as well as by the manner in which they affected their feelings, and the thermometer, until they reached the regions of perpetual froſt, and the vaſt collections of ſnow, which are but little waſted there by the melting, but continue to accumulate annually, until they overhang the precipices of the mountains, and fall at laſt with a dreadful ruin on the country that is below them.

The ſame perpetual froſt, and accumulation of ſnows, with the conſequences of it, are well known to take place on all other high mountains, ſuch as the Alps and Pyrenees; and the coldneſs of the ſuperior parts of the atmoſphere has been further experienced by thoſe who have aſcended from the plains to a great height, by the uſe of baloons. To all thoſe proofs, we may add the ſhowers of hail which

ſometimes fall in the heat of ſummer, and the piercing cold with which theſe are accompanied, in conſequence of their mixing a part of the ſuperior with the inferior air.

It is, therefore, plain that only the lower region of the air is warm, the heat being found to diminiſh while we aſcend in the atmoſphere, and that the very high regions of it are exceſſively cold. This has long been known, and attempts have been made to explain how it happens.

The explanation that has been commonly given is, that the light of the ſun reflected from the ſides of the mountains, and other elevated parts, into the valleys and plains, is the cauſe of the greater heat of the air in theſe. But it is eaſy to ſhew that this opinion is a great miſtake. If the declivities of the mountains, and other elevated parts of the earth, had poliſhed ſurfaces, the light would be reflected in the manner ſuppoſed. But the ſurface of the earth is every where compoſed of rough uneven materials, which abſorb the greater part of the light that falls on them, and diſperſe the reſt in every poſſible direction; the conſequence of which is the contrary to what is here ſuppoſed; for the mountains muſt receive more reflected or diſperſed light from the plains, than that which the plains can receive from the mountains. The quantity of light diſperſed from the ſurface of mountains and plains, is in general proportional to the extent of thoſe ſurfaces; and plains are generally far more extenſive than thoſe declivities of mountains, from which they can receive diſperſed light; and, moreover, the greateſt number of extenſive plains are at ſuch a diſtance from mountains, that it cannot be ſuppoſed that they owe their ſuperior heat to light reflected from theſe. There are parts of them, from which the mountains are at ſuch a diſtance that they cannot be ſeen, and yet theſe parts are as warm, and generally warmer, than thoſe which are in the neighbourhood of the mountains.

The principles from which a good explication of this phenomenon may be deduced, are two well known qualities of the atmoſphere,—its

tranſparency, and its elaſticity or compreſſibility. A conſequence of the tranſparency of the atmoſphere is, that the light of the ſun, in paſſing through it to the ſurface of the earth, does not communicate heat to it, or communicates only a very ſmall quantity, eſpecially in the ſuperior regions, which are much more clear and tranſparent than the inferior. It is the nature of light to excite heat, chiefly in opaque bodies, which ſtop its progreſs and abſorb it; but very little in thoſe that are tranſparent, and allow it to paſs through them. To be ſatisfied of this, with reſpect to air, we need only to reflect on our own experience. We have all felt, that in a clear day in the ſpring ſeaſon, or in the beginning of ſummer, the air is often very cool, although the ſun ſhines bright, and has great power to warm the ſolid and opaque bodies which are expoſed to his rays. Stones, ſlate, plates of metal, and other dry and opaque bodies, will become warmer than the human body, though the air at the ſame time feels remarkably cold. We have another ſtrong proof of this, when we attend to the focus of a large burning glaſs, or mirror, expoſed to the ſun. Although any ſolid or opaque body, introduced into the ſpot, is melted or diſſipated, or conſumed in an inſtant,—before it is put there, we cannot, by looking at the place, perceive any ſigns of that intenſe and terrible heat by which it is ſo quickly affected. The truth is, that there is no great heat in that ſpot, until the opaque body is placed in it. There is a great quantity of light paſſing through that place, but the air being tranſparent, little heat is produced. There is another experiment, by which the effect of tranſparency, in a medium tranſmitting light, is further demonſtrated. If a large lens be expoſed to the ſun, and a tub of clear water be placed below it, ſo ſituated that the focus of the lens ſhall be formed below the ſurface of the water, and in the centre of its maſs, there are no remarkable ſigns of great heat produced in that focus; but let a ſtick be thurſt down, and introduced into that ſpot, the water will ſoon boil on the ſurface of the ſtick, and the internal parts of the wood will be ſcorched to a black coal, by the in-

ſenſe light which penetrates into them, and acts ſo powerfully upon the ſurface.

It is therefore evident that the ſun warms the atmoſphere, by firſt warming the ſurface of the earth, from which the heat is communicated to the air immediately above it; and, no doubt, it muſt act alſo on the vapours and effluvia which diminiſh the tranſparency of the lower region of the atmoſphere. But its greateſt action is on the earth. We can perceive this in clear ſunſhine, and when the ſun is high, by looking at diſtant objects over the ridge of a riſing ground. Thoſe objects, or their parts, appear to have a tremulous motion, the light which comes from them to the eye being diſturbed in its courſe by the riſing of warm air, which is conſtantly receiving heat from the ſurface of the ſoil.

Thus we have accounted for the heat being found chiefly in the inferior region of the atmoſphere: We muſt next explain why it remains in a great meaſure there, and is not ſo quickly communicated to the ſuperior regions as we might expect, when we conſider the expanſibility of the air, and the motions which heat applied to it commonly produces.

This partial diſtribution and confinement, in ſome meaſure, of heat, to the lower region of the atmoſphere, depends on the elaſticity and compreſſibility of the air. A conſequence of this quality is, that the lower region of it is condenſed by the preſſure of the ſuperior parts; and, although it be expanded by the heat in it, and thus made rarer than it would otherwiſe be, this expanſion is not equal to the condenſation it ſuffers by the preſſure of the ſuperior parts *. The lower air, therefore, although heated and expanded, continues ſtill denſer than the ſtrata of the atmoſphere that are conſiderably above it, and is

* When we deſcend from high mountains, the air is found warmer by one degree for every 200 feet of perpendicular deſcent; but the preſſure of 200 feet perpendicular of the atmoſphere, has two and a-half times as much effect in condenſing the air, as one degree of heat has in rarefying it.

not diſpoſed to be preſſed or driven far upwards in them, but remains in its place near the ſurface of the earth, where the heat is moſt uſeful for the production and ſupport of vegetables and animals *. Thus we are enabled to underſtand the excellent effects of a proper degree of ſhelter in increaſing the fertility of a country, and the comfort of its inhabitants. Whatever preſerves the lower regions of the air from frequent and violent agitation, improves the climate in our northern latitudes, by preventing more or leſs the chilling effects of wind, and diſperſion of the warmth accumulated at the ſurface of the earth. This ſhelter is procured, to a certain degree, by a due proportion of plantations of timber trees, properly diſpoſed over the face of a country, ſo as to break or diminiſh the force of the winds.

If it ſhould be here aſked, what is meant by the lower region of the air, and what is the limit between it and higher regions? the anſwer muſt be, that there is no preciſe limit; but that, in general, by the lower region, I mean the parts of the atmoſphere which do not reach to an elevation equal to that of high mountains. Very few of the higheſt mountains riſe to the height of two miles above the ſurface of the ſea; but we know for certain, by the appearance of ſome meteors, as well as by calculation, that our atmoſphere exiſts, and is capable of action at the height of fifty miles, though at that height, by the want of preſſure, it is in a highly rarefied ſtate.

Even the lower region, as above pointed out, is very different in different parts of it; thoſe parts neareſt the earth, and in the loweſt ſituations, being in general the warmeſt, as well as the moſt compreſſed; while the heat of thoſe above them, and the preſſure to which they are expoſed, gradually diminiſhes.

* The celebrated Dr. Franklin explained, in much the ſame manner, the unequal diſtribution of heat in the atmoſphere; and deduced from theſe principles the phenomena of whirlwinds, hurricanes, and water-ſpouts, which never before were ſo ingeniouſly and ſatisfactorily accounted for. See Phil. Tranſ. vol. 55,—and Experiments and Obſervations on Electricity, &c. by Benjamin Franklin, p. 182 and 216.

The diminution of the heat is about one degree of Fahrenheit for every two hundred feet of elevation, and the multiplication of bulk is proportional (very nearly) to the elevation of temperature. The greateſt difference of heat, therefore, balances two-fifths of the effect of compreſſion, and cannot, alone, raiſe the air contiguous to the earth's ſurface to any conſiderable height. Were it not for the winds, the region of perpetual ſnow, even under the equator, would not be a mile from the level of the ſea. Thus, there is a gradual and undefinable tranſition from the warmth of the lower air to the coldneſs of the higher; and, in the higher regions, the ſame gradual diminution of heat and compreſſion takes place.

This view of our atmoſphere enables us to underſtand why thoſe horizontal motions of the air, which we call winds, and which have ſome effect in mixing the ſuperior colder with the inferior warmer air, produce this effect in a far leſs degree than might be apprehended, although it often happens that they have different directions in the different ſtrata of the atmoſphere at the ſame time; for the ſtrata, which differ greatly in their heat, may eaſily move horizontally in different directions, without mixing together, being ſeparated by a multitude of intermediate ſtrata, in which there is a regular and ſlow gradation of denſity and heat. We were led to give the preceding views of the communication, diſtribution, and progreſſion of heat, as examples of ſome of the improvements we have made in our knowledge of heat, ſince the invention of thermometers: And this invention aroſe from the diſcovery of the expanding power of heat.

Having, therefore, now done with the conſideration of expanſion, we proceed to conſider the next of the general effects of heat, formerly enumerated, which is fluidity.

SECT. II.—OF FLUIDITY.

THE propriety of conſidering fluidity as one of the general effects of heat, is ſufficiently obvious. It is well known that a great number of different ſubſtances, which commonly appear in a ſolid ſtate, can be rendered fluid by heat, and will continue, ſo long as they continue heated to the proper degree. But, *beſides this conſideration*, we find reaſon to be perſuaded, that fluidity, wherever we meet with it in the various kinds of matter, is *always* the effect or conſequence of heat; experience, in moſt caſes, having taught us, that thoſe bodies which commonly appear in a fluid ſtate, and the fluidity of which might be thought a natural and eſſential quality, do nevertheleſs derive this fluidity from the quantity of heat they contain; for, when we diminiſh their heat ſufficiently, they do not fail to aſſume a ſolid form; from which it is plain that the fluid ſtate in which we commonly ſee them depends upon heat; nor is there any other circumſtance, ſo far as we can obſerve, that is neceſſary and indiſpenſable to their fluidity.

In general, therefore, we have the ſtrongeſt reaſons for conſidering heat as the efficient cauſe of fluidity, in all bodies in which this quality is found.

But, as there is a great diverſity among different bodies, with reſpect to the intenſity of heat neceſſary to produce this effect on them, this ſhews that there are certain peculiarities in their conſtitution, which increaſe in ſome, and diminiſh in others, the diſpoſition to fluidity; and theſe peculiarities are plainly certain differences in the nature, number, and compoſition of their conſtituent parts; a proof of which is, that we can by mixture, in moſt caſes, change a body from its ordinary ſtate, in this reſpect. Water, for example, by having ſalt or ſpirit of wine mixed with it, acquires an increaſe of its diſpoſition to fluidity, and is enabled to endure, without freezing, a

more intenſe cold than it can bear in its ordinary ſtate. But ſtill its fluidity, however much it may reſiſt the cold, depends upon heat; for, by continuing to take heat from it, it will freeze at laſt. Moreover, as long as it does not undergo any new change with reſpect to mixture, no other cauſe but a certain quantity of heat alone can render and preſerve it fluid.

Our experience of the freezing or conſolidation of fluids, when expoſed to more or leſs powerful degrees of cold, is almoſt univerſal. The exceptions are very few. The ſtrongeſt ſpirit of wine, and a few ſubtile and volatile oils, are the only ſubſtances which have not yet been congealed by any degree of cold hitherto known. As theſe, however, are ſo few in number, it appears unreaſonable to believe that they have a nature and conſtitution ſo different from that of all other bodies, that fluidity is in them an eſſential quality, of which they cannot be deprived by any diminution of their heat. As we have no certain knowledge of what is the loweſt poſſible degree of heat, or moſt extreme intenſity of cold, but, on the contrary, ſhall have reaſon hereafter to be perſuaded that the moſt violent cold which has yet been obſerved, is very far ſhort of the moſt extreme degree, it is more reaſonable to ſuppoſe that theſe few bodies differ from others, only in having a much greater *diſpoſition* to fluidity, and therefore as requiring leſs heat to preſerve them fluid; in conſequence of which, we have never yet known a degree of cold ſufficient for congealing them; but that they would undoubtedly congeal, like other fluids, were they expoſed to a ſufficient cold.

Quickſilver was, not long ſince, one of this ſmall number of bodies, which, having never been ſeen in any other than a fluid ſtate, was conſidered as naturally and eſſentially fluid, and incapable of being reduced alone to a ſolid form, until experiments were made with it, firſt in different parts of the Ruſſian empire, ſince the year 1760, and verified afterwards in other places. By theſe experiments, every perſon muſt be convinced that quickſilver is a metal, which can

become ſolid and malleable like the reſt, but that it is melted with ſo little heat, that the loweſt degree ever obſerved, over the greater part of the ſurface of this earth, is more than ſufficient to preſerve it fluid.

In the ſame manner may we conſider all other fluids as ſolids melted by heat.

Some philoſophers, however, have offered many objections againſt admitting this general propoſition concerning the nature of fluids. They thought it neceſſary to ſuppoſe that water is an exception. They could not be perſuaded that in it fluidity is the effect of heat, but ſuppoſed this quality to be an eſſential quality of the water, depending on the ſpherical form and poliſhed ſurface of its particles, and that the freezing of it depends on the introduction of ſome extraneous matter. This opinion is defended by Profeſſor Muſchenbroeck, in his phyſics *, and he has collected all the reaſons and arguments which have been deviſed for ſupporting ſuch an opinion.

But, when we conſider theſe reaſons and arguments with due attention, we do not find that any of them are valid.

Many of them are alleged facts, adduced to prove that water is frozen on ſome occaſions, and in ſome circumſtances, under which Mr. Muſchenbroeck did not comprehend how it ſhould be cooled to the degree reckoned neceſſary to its congelation; and he therefore concludes that the freezing of it muſt proceed from ſome other cauſe than the diminution of its heat to the proper degree.

But had he applied a good thermometer to it, he would have found that it was actually cooled to the uſual degree in every caſe in which it was frozen. For the truth of this we may depend on the teſtimony of Dr. Martin, who, with the aſſiſtance of his friends, took care to have experiments made, in many diſtant parts of the world, and at different times, with thermometers, on the goodneſs of which he could entirely rely. There is no doubt that many miſtakes have been

* Muſchenb. Phyſ. de aqua.

committed by uſing bad ones, or by the want of ſkill to uſe them properly. But the truth is, that in the facts adduced by Muſchenbroeck, no thermometer whatever was applied to the water itſelf, but only vague reaſoning is employed, to make it appear probable that it was not cooled to the proper degree. We may, therefore, paſs over the greater number of his reaſons, and take ſome notice of a few only of his facts, which are ſurpriſing in themſelves, and could not be explained by any principles then known, and which beſides are ſtated by him in ſuch a manner as to make them appear uncommonly perplexing.

The fact which firſt induced him to think that ice is formed by the entrance of extraneous matter into water, is the enlargement of bulk; ice being always more bulky than the water from which it was produced, in the proportion of about 9 to 8. This enlargement of bulk is the effect of a powerful cauſe acting with great force, which has been ſhewn by many experiments. "Now he thinks it more proba-
" ble that this enlargement of bulk, and the force with which it is
" performed, depend on the entrance of ſome ſubtile matter, than on
" the ſimple diminution of heat, the general effect of which is to oc-
" caſion contraction in bodies."

To the whole of this argument, however, it is eaſy to anſwer, that the fact is better explained by the ſuppoſition of a particular modification of the attraction, which makes the parts of the water unite and cohere, in one way rather than in any other.

The attraction of coheſion, which, in this caſe, is the attraction of the particles of the water for one another, is undoubtedly the cauſe of the concretion of water into ice, as it is the cauſe of the ſolidity and hardneſs of all other bodies. No perſon doubts that the hardneſs of iron is owing to the coheſive attraction of its particles for one another. We can make theſe particles change their poſition among one another, by hammering the iron, or drawing it into wire, and they ſtill cohere. And yet, when iron that has been melted is allowed, by cool-

ing, to reſume its ſolidity, which it does when it is yet in a ſtrong red heat, the ſame effect is produced as in the caſe of water; the iron, when ſolid being a little more bulky than when it was fluid. And the reaſon is the ſame in both caſes,—the attraction of coheſion, which which was counteracted and overcome in theſe bodies, by the repulſive power of heat; being, in conſequence of the diminution of the heat, allowed to operate, it acts in a peculiar manner, forcing the particles to aſſume an arrangement different from that which they had while the body was fluid, and which requires a little more ſpace. This peculiar action of the attraction of coheſion in ſuch bodies, I formerly compared to that of a great number of ſmall magnets ſet afloat on the ſurface of water. There is no doubt that by the attraction of their north and ſouth poles, they would arrange themſelves in a particular manner, different from that of the cloſeſt arrangement; and that ſomething like this does actually happen in freezing water, and congealing iron, is ſufficiently evident from a number of phenomena formerly deſcribed.

Another remark of Profeſſor Muſchenbroeck is, that " in moderate " froſty weather, water, expoſed and preſerved from diſturbance, re- " mains fluid, but being then diſturbed, it ſuddenly freezes." To this it may be anſwered, that there is here no appearance which gives reaſon for concluding that the freezing of it depends on the introduction of extraneous matter. And when we conſider the experiment more fully, which is to be done in the ſequel, we ſhall find that it can be better explained, in a very different manner.

3*dly*, " A wet cloth hung in the air, in cold and dry weather, will " ſometimes become ſtiff, by the freezing of the humidity in it, when " the thermometer ſhews the cold of the air to be inſufficient for the " freezing of water in ordinary circumſtances." And Mr. Muſchenbroeck ſuppoſed, " that, in this caſe, the ice was formed in the cloth " by ſome other cauſe than the diminution of heat, and the power of " attraction." But he formed this judgment from the indication of

the thermometer hung in the air. Had he applied it to the cloth itſelf, he would have found it cooled to the uſual degree of 32 Fahrenheit; and the truth is, that a wet cloth, hung up in theſe circumſtances, becomes colder than any other body around it; which fact ſhall be explained hereafter.

4*thly*, "Water, at 33 degrees of Fahrenheit, when the ſtrong ni-
"trous acid is added to it, becomes warmer to the 41ſt degree. But,
"if we pour the ſame ſtrong acid on pounded ice, at 32 degrees of
"Fahrenheit, an enormous cold is the conſequence. How can theſe
"different effects be produced, when the heat of the water and ice
"are ſo nearly the ſame, unleſs there be in the ice ſome heterogeneous
"matter, the power of which is increaſed by the acid?"

To this we can anſwer, that ice is certainly different from water, and produces different effects, when mixed with ſome other ſubſtances; and the queſtion is, In what does this difference conſiſt? Mr. Muſchenbroeck ſays that it conſiſts in the preſence of frigorific particles in the ice; but we ſhall ſoon give another account of it, that ſhall explain this experiment, without having recourſe to ſuch a ſuppoſition.

5*thly*, He concludes, "That in ice the particles of water have cor-
"puſcles of a different kind mixed with them; for this reaſon, that
"the water produced by the melting of ice, is unfit for boiling vege-
"tables tender, or for drawing a good infuſion from tea, coffee, &c.
"until it has been boiled for ſome time."

In this reaſon, it is ſtated as a fact, that the water of melted ice is different from good and ſoft water, but that it becomes ſoft and good by boiling it ſome time. Now, this fact appears exceedingly improbable, and before it be admitted, would need to be aſcertained by accurate and deciſive experiments; for, in whatever manner ice may be different from water, that difference ſhould certainly be removed when the ice is changed again into water. The Profeſſor does not mention any accurate experiments made by himſelf or others, to prove this al-

leged fact, and ſome I have made diſprove it entirely. I am perſuaded that the opinion of its truth is vulgar and erroneous, and has ariſen from the coldneſs of the water which comes from ice; for it is well known, that very cold water applied to ſoap, has little power to diſſolve it, or appears to be hard water, when compared with the ſame water leſs cold; and it is a maxim in cookery, that water employed for the boiling of vegetables does not perform its office well, when the vegetables are put into it in its cold ſtate, although it had not been frozen for a long time before. It may therefore be preſumed, that the vulgar opinion of the unfitneſs of ice-water to diſſolve ſoap, or boil vegetables well, or to draw good infuſions from vegetable ſubſtances, has ariſen from its being applied to them colder than other water.

6thly, He ſtates as fact, "That when a mixture is made of ſnow "and common ſalt in a proper veſſel, and in the middle of this mix-"ture another is placed, containing pure water; if we now ſet the "whole apparatus upon a fire, while the ſalt is diſſolved, and the ſnow "melts, the pure water is frozen, and the fire accelerates the freezing "of the pure water; for the ſtronger the fire is, the pure water is "frozen ſo much the ſooner, which (he adds) could not happen, if "the fire did not chaſe the congealing particles from the ſnow into "the water; and he cannot believe that any perſon will aſſert, that, "in this caſe, the fire chaſes an abſence or deficiency of heat from the "ſnow into the water."

It muſt be confeſſed, that this experiment, thus ſtated, appears very perplexing; but this proceeds from the introduction of a circumſtance into it, which is no way neceſſary to its ſucceſs, and is introduced for no other reaſon but to make it appear more ſurpriſing. The circumſtance I mean, is ſetting the whole apparatus upon a fire. The plain truth is, that the ſalt and ſnow act on one another in a way hereafter to be explained; and, in conſequence of this action, they unite together, and are both liquefied, becoming at the ſame time intenſely colder than they were before. This will be ſure to happen, and with

more effect, though we do not ſet the apparatus upon a fire. But the power of this mixture, when made in proper quantity, is ſo great, that although the heat received from the fire diminiſhes the cold produced, there is ſtill enough for freezing the water, which, when frozen, is always as cold as in ordinary caſes. There is only one caſe in which the application of heat can promote or quicken the action of the mixture, and this happens when the ſnow and ſalt, before they are mixed, are both as cold as the Zero in Fahrenheit's ſcale, or are near to this cold. When ſo cold as this, they are little diſpoſed to act upon one another; and if heat be applied to them, it quickens their action. But even in this caſe, the heat applied diminiſhes their power, upon the whole, to produce cold; for, if time be allowed, they will be ſufficient for congealing a larger quantity of water, without the application of heat to them, than when it is applied. And this experiment is to be explained in the ſequel, without employing the ſuppoſition of congealing or frigorific particles.

Upon the whole, there are many of Profeſſor Muſchenbroeck's reaſons for his opinion on the congelation of water, that are quite inconcluſive. And though ſome muſt appear, for the preſent, unaccountable facts, theſe are hereafter to be explained. But, in the mean time, it is evident that none of them give any ſatisfactory proof that the fluidity of water is an eſſential quality, or that any new matter is introduced into it when it is frozen. The propenſity which many have to imagine water is a ſubſtance naturally and eſſentially fluid, is a prejudice contracted from the habit of ſeeing it often every day in a fluid ſtate, or much oftener in this ſtate than in the ſtate of a ſolid.

As we have ſufficient reaſon, therefore, for believing that fluidity is in all caſes the effect of heat, and not an eſſential quality of any of the palpable kinds of matter, ſo it appears to be one of thoſe effects which are produced generally, and upon all kinds of bodies. There are very few ſubſtances now known, which cannot, in ſome degree of heat or other, be brought into a ſtate of fluidity by heat. Thoſe that

are the leaſt diſpoſed to it, are the earths and ſtones, many of which were formerly reckoned incapable of being melted; but later experiments have ſhewn this opinion to have been erroneous, eſpecially ſome experiments with burning glaſſes, and ſome made in conſequence of improvements in the conſtruction of furnaces, and in the management of fuel, by which means much more violent heats have been produced than formerly, and which have brought into perfect fuſion many of thoſe bodies which were reckoned incapable of being melted by heat.

In conſidering this effect of heat, we may firſt remark one obvious difference between it and expanſion, which is, that whereas expanſion is, in general, an equable and regular effect, accompanying every increaſe of the heat of bodies, the change of a body from the ſtate of ſolidity to that of fluidity, does not take place, except when its heat is increaſed, ſo as to riſe above a particular degree. This has been aſcertained by innumerable experiments made with thermometers. If the heat of the body be increaſed above this degree, it becomes fluid in all the higher degrees. If again its heat be diminiſhed, ſo as to fall below this degree, it becomes ſolid, and remains ſolid in all the lower degrees. This at leaſt may be ſtated as the general fact.

There are, however, many ſubſtances in which the tranſition is not ſo ſudden. Theſe, in a certain latitude of heat, are reduced to an intermediate ſtate, a ſtate of ſoftneſs, and paſs through all the degrees of it, while they are changing from a ſolid to a fluid, by heat. We have examples of this in bees wax, reſin, tallow, glaſs, and many other ſubſtances. But, even in theſe, every degree of ſoftneſs depends on a correſponding degree of heat, which has the power to produce it; and, in moſt of theſe bodies, there is alſo a remarkable ſtep from the greateſt degree of ſoftneſs to perfect fluidity, which always depends on a certain intenſity of heat, neceſſary to the perfect liquefaction of that particular body.

We may therefore ſay in general, that each different kind of mat-

ter requires to be heated to a particular degree, or above it, to render it fluid, and that below this degree it is always ſolid, or has ſome ſolidity. This degree of heat is therefore called the CONGEALING, or the MELTING POINT of ſuch body. It is called the congealing point of ſuch bodies as appear commonly in a ſtate of fluidity, and the melting point of thoſe that are ſolid, in ordinary circumſtances. This point, or intenſity of heat, is invariably the ſame for each particular kind of matter, no other circumſtance but heat appearing to have the ſmalleſt influence in producing fluidity, ſo long as the body itſelf continues the ſame, unaltered in the mixture or proportion of its conſtituent parts. But, when we compare different kinds of matter, there is all the variety that can be imagined between thoſe that require for their fuſion the utmoſt violence of heat, and thoſe for which ſo little is ſufficient, that they always appear, in ordinary circumſtances, in a fluid form.

Among this variety there are a number, the liquefaction of which by heat is diſtinguiſhed by the term of VITRIFICATION.

This term is applied to the fuſion of thoſe earthy bodies which undergo a certain change of their appearance and texture, by being melted and allowed again to congeal. Whatever might be their appearance before, in their natural ſtate, they aſſume, after fuſion, a reſemblance of glaſs, forming a hard brittle maſs, which breaks with equal eaſe in every direction, and into fragments, the ſurfaces of which are ſmooth, poliſhed, and undulated. When an earthy ſubſtance aſſumes this form after fuſion, it is ſaid to be VITRIFIED.

Thus we have taken a general view of the ſubject, according to the notions which were formerly had of it.

I muſt, however, now add, that this account of fluidity, as an effect of heat, is not complete and ſatisfactory, if it be underſtood in conformity with the common opinions which were entertained of it. It is inconſiſtent with many remarkable phenomena; and theſe

phenomena, when attentively conſidered, ſhew that fluidity is produced by heat, in a very different manner from that which was commonly imagined; a manner, however, which, when underſtood, enables us to explain many particulars relating to heat or cold, which appeared in the former view of the ſubject, quite perplexing and unaccountable.

Fluidity was univerſally conſidered as produced by a ſmall addition to the quantity of heat which a body contains, when it is once heated up to its melting point; and the return of ſuch body to a ſolid ſtate, as depending on a very ſmall diminution of the quantity of its heat, after it is cooled to the ſame degree; that a ſolid body, when it is changed into a fluid, receives no greater addition to the heat within it than what is meaſured by the elevation of temperature indicated after fuſion by the thermometer; and that, when the melted body is again made to congeal, by a diminution of its heat, it ſuffers no greater loſs of heat than what is indicated alſo by the ſimple application to it of the ſame inſtrument.

This was the univerſal opinion on this ſubject, ſo far as I know, when I began to read my lectures in the Univerſity of Glaſgow, in the year 1757. But I ſoon found reaſon to object to it, as inconſiſtent with many remarkable facts, when attentively conſidered; and I endeavoured to ſhew, that theſe facts are convincing proofs that fluidity is produced by heat in a very different manner.

I ſhall now deſcribe the manner in which fluidity appeared to me to be produced by heat, and we ſhall then compare the former and my view of the ſubject with the phenomena.

The opinion I formed from attentive obſervation of the facts and phenomena, is as follows. When ice, for example, or any other ſolid ſubſtance, is changing into a fluid by heat, I am of opinion that it receives a much greater quantity of heat than what is perceptible in it immediately after by the thermometer. A great quantity of heat enters into it, on this occaſion, without making it apparently warmer,

when tried by that inſtrument. This heat, however, muſt be thrown into it, in order to give it the form of a fluid; and I affirm, that this great addition of heat is the principal, and moſt immediate cauſe of the fluidity induced.

And, on the other hand, when we deprive ſuch a body of its fluidity again, by a diminution of its heat, a very great quantity of heat comes out of it, while it is aſſuming a ſolid form, the loſs of which heat is not to be perceived by the common manner of uſing the thermometer. The apparent heat of the body, as meaſured by that inſtrument, is not diminiſhed, or not in proportion to the loſs of heat which the body actually gives out on this occaſion; and it appears from a number of facts, that the ſtate of ſolidity cannot be induced without the abſtraction of this great quantity of heat. And this confirms the opinion, that this quantity of heat, abſorbed, and, as it were, concealed in the compoſition of fluids, is the moſt neceſſary and immediate cauſe of their fluidity.

To perceive the foundation of this opinion, and the inconſiſtency of the former with many obvious facts, we muſt conſider, in the firſt place, the appearances obſervable in the melting of ice, and the freezing of water.

If we attend to the manner in which ice and ſnow melt, when expoſed to the air of a warm room, or when a thaw ſucceeds to froſt, we can eaſily perceive, that however cold they might be at the firſt, they are ſoon heated up to their melting point, or begin ſoon at their ſurface to be changed into water. And if the common opinion had been well founded, if the complete change of them into water required only the further addition of a very ſmall quantity of heat, the maſs, though of a conſiderable ſize, ought all to be melted in a very few minutes or ſeconds more, the heat continuing inceſſantly to be communicated from the air around. Were this really the caſe, the conſequences of it would be dreadful in many caſes; for, even as things are at preſent, the melting of great quantities of ſnow and ice occaſions

violent torrents, and great inundations in the cold countries, or in the rivers that come from them. But, were the ice and ſnow to melt as ſuddenly as they muſt neceſſarily do, were the former opinion of the action of heat in melting them well founded, the torrents and inundations would be incomparably more irreſiſtible and dreadful. They would tear up and ſweep away every thing, and that ſo ſuddenly, that mankind ſhould have great difficulty to eſcape from their ravages. This ſudden liquefaction does not actually happen; the maſſes of ice or ſnow melt with a very ſlow progreſs, and require a long time, eſpecially if they be of a large ſize, ſuch as are the collections of ice, and wreaths of ſnow, formed in ſome places during the winter. Theſe, after they begin to melt, often require many weeks of warm weather, before they are totally diſſolved into water. This remarkable ſlowneſs with which ice is melted, enables us to preſerve it eaſily during the ſummer, in the ſtructures called Ice-houſes. It begins to melt in theſe, as ſoon as it is put into them; but, as the building expoſes only a ſmall ſurface to the air, and has a very thick covering of thatch, and the acceſs of the external air to the inſide of it is prevented as much as poſſible, the heat penetrates the ice-houſe with a ſlow progreſs, and this, added to the ſlowneſs with which the ice itſelf is *diſpoſed* to melt, protracts the total liquefaction of it ſo long, that ſome of it remains to the end of ſummer. In the ſame manner does ſnow continue on many mountains during the whole ſummer, in a melting ſtate, but melting ſo ſlowly, that the whole of that ſeaſon is not a ſufficient time for it complete liquefaction.

This remarkable ſlowneſs with which ice and ſnow melt, ſtruck me as quite inconſiſtent with the common opinion of the modification of heat, in the liquefaction of bodies.

And this very phenomenon is partly the foundation of the opinion I have propoſed; for if we examine what happens, we may perceive that a great quantity of heat enters the melting ice, to form the water into which it is changed, and that the length of time neceſſary for

the collection of ſo much heat from the ſurrounding bodies, is the reaſon of the ſlowneſs with which the ice is liquefied. If any perſon entertain doubts of the entrance and abſorption of heat in the melting ice, he needs only to touch it; he will inſtantly feel that it rapidly draws heat from his warm hand. He may alſo examine the bodies that ſurround it, or are in contact with it, all of which he will find deprived by it of a great part of their heat; or if he ſuſpend it by a thread, in the air of a warm room, he may perceive with his hand, or by a thermometer, a ſtream of cold air deſcending conſtantly from the ice; for the air in contact is deprived of a part of its heat, and thereby condenſed and made heavier than the warmer air of the reſt of the room; it therefore falls downwards, and its place round the ice is immediately ſupplied by ſome of the warmer air; but this, in its turn, is ſoon deprived of ſome heat, and prepared to deſcend in like manner; and thus there is a conſtant flow of warm air from around, to the ſides of the ice, and a deſcent of the ſame in a cold ſtate, from the lower part of the maſs, during which operation the ice muſt neceſſarily receive a great quantity of heat.

It is, therefore, evident, that the melting ice receives heat very faſt, but the only effect of this heat is to change it into water, which is not in the leaſt ſenſibly warmer than the ice was before. A thermometer, applied to the drops or ſmall ſtreams of water, immediately as it comes from the mleting ice, will point to the ſame degree as when it is applied to the ice itſelf, or if there is any difference, it is too ſmall to deſerve notice. A great quantity, therefore, of the heat, or of the matter of heat, which enters into the melting ice, produces no other effect but to give it fluidity, without augmenting its ſenſible heat; it appears to be abſorbed and concealed within the water, ſo as not to be diſcoverable by the application of a thermometer.

In order to underſtand this abſorption of heat into the melting ice, and concealment of it in the water, more diſtinctly, I made the following experiments.

The plan of the firſt was, to take a maſs of ice, and an equal quantity of water, in ſeparate veſſels, of the ſame ſize and ſhape, and as nearly as poſſible of the ſame heat, to ſuſpend them in the air of a warm room, and, by obſerving with a thermometer the celerity with which the water is heated, or receives heat, to learn the celerity with which it enters the ice; and the time neceſſary for melting the ice being alſo attended to, to form an eſtimate, from theſe two data, of the quantity of heat which enters the ice during its liquefaction.

In order to prepare for this experiment, I choſe two thin globular glaſſes, four inches diameter, and very nearly of the ſame weight: I poured into one of them five ounces of pure water, and then ſet it in a mixture of ſnow and ſalt, that the water might be frozen into a ſmall maſs of ice. As ſoon as frozen, it was carried into a large empty hall, in which the air was not diſturbed nor varied in its temperature during the progreſs of the experiment; and in this room the glaſs was ſupported, as it were, in mid air, by being ſet on a ring of ſtrong wire, which had a tail iſſuing from the ſide of it five inches long, and the end of this tail was fixed in the moſt projecting part of a reading deſk or pulpit: And in this ſituation the glaſs remained until the ice was completely melted.

When the ice was thus placed, I ſet up the other globular glaſs preciſely in the ſame ſituation, and at the diſtance of 18 inches to one ſide, and into this I poured five ounces of water, previouſly cooled, as near to the coldneſs of melting ice as poſſible, viz. to 33 degrees, and ſuſpended in it a very delicate thermometer, the bulb of which being in the centre of the water, and the tube being ſo placed, that without touching the thermometer, I could ſee the degree to which it pointed. I then began to obſerve the aſcent of this thermometer, at proper intervals, in order to learn with what celerity the water received heat, ſtirring the water gently with the end of a feather about a minute before each obſervation. The heat of the air, examined at a little diſtance from the glaſſes, was 47 degrees of Fahrenheit's ſcale.

The thermometer aſſumed the temperature of the water in leſs than half a minute, after which, the riſe of it was obſerved every five or ten minutes, during half an hour. At the end of that time, the water was grown warmer than at firſt, by ſeven degrees; and the temperature of it had riſen to the 40th degree of Fahrenheit's ſcale.

The glaſs with the ice was, when firſt taken out of the freezing mixture, four or five degrees colder than melting ſnow, which I learned by applying the bulb of the thermometer to the bottom of it; but after ſome minutes, it had gained from the air thoſe four or five degrees, and was juſt beginning to melt, which point of time was then noted, and the glaſs left undiſturbed ten hours and a half. At the end of this time, I found only a very ſmall and ſpongy maſs of the ice remaining unmelted, in the centre of the upper ſurface of the water, but this alſo was totally melted in a few minutes more; and, introducing the bulb of the thermometer into the water, near the ſides of the glaſs, I found the water there was warmed to the 40th degree of Fahrenheit. From this it appears, that when a conſiderable part of the ice was melted, and the quantity of water around it was increaſing, the heat was not tranſmitted through this water to the remaining ice altogether ſo faſt as it was received by the water; which is eaſily underſtood, if we conſider that the diſtance between the remaining ice and the external ſurface of the veſſel through which the heat entered, was gradually increaſing, and that heat always requires time to paſs through bodies, or to be communicated from one part of them to another *.

It appears, therefore, from this experiment, that it was neceſſary that the glaſs with the ice receive heat from the air of the room during

* Water, while its heat is increaſed from the 33d to the 40th degree of Fahrenheit, ſuffers very little or no expanſion from the heat entering it (Vide De Luc, *Recherches*, &c. p. 418.) The water, therefore, in this caſe, would not be made to move and circulate by the unequal heat of the different parts of it, but would remain in a great meaſure at reſt.

21 half-hours, in order to melt the ice into water, and to heat that water to the 40th degree of Fahrenheit. During all this time, it was receiving the heat, or matter of heat, with the ſame celerity (very nearly) with which the water-glaſs received it, during the ſingle half-hour in the firſt part of the experiment. For, as the water received it with a celerity which was diminiſhing gradually during that half-hour, in conſequence of the diminution of difference between its degrees of heat and that of the air; ſo the glaſs with the ice alſo received heat with a diminiſhing celerity, which correſponded exactly with that of the water-glaſs, only that the progreſſion of this diminution was much more ſlow, and correſponded to the whole time which the water ſurrounding the ice required to become warmed to the 40th degree of Fahrenheit. The whole quantity of heat, therefore, received by the ice-glaſs during the 21 half-hours, was 21 times the quantity received by the water-glaſs during the ſingle half-hour. It was, therefore, a quantity of heat, which, had it been added to liquid water, would have made it warmer by $\overline{40-33}\times 21$, or 7×21, or 147 degrees. No part of this heat, however, appeared in the ice-water, except 8 degrees; the remaining 139 or 140 degrees had been abſorbed by the melting ice, and were concealed in the water into which it was changed.

The communication of heat to the melting ice was eaſily perceived, during the whole time of its expoſition, by feeling the ſtream of cold air which deſcended from the glaſs.

This experiment was an analyſis of the manner in which ice is melted by the heat of the air in ordinary circumſtances.

But another obvious method of melting ice occurred to me, in which it would be ſtill more eaſy to perceive the abſorption and concealment of heat, and this was by the action of warm water.

When hot and cold water are mixed together, the exceſs of heat contained in the hot water is equally diſtributed in an inſtant through the whole mixture, and raiſes the temperature of it according to the greatneſs of the exceſs of temperature, and the proportion which the

hot water bore to the cold. If the quantities of hot and cold water are equal, the temperature is the middle degree between that of the hot and that of the cold. No part of the heat diſappears on this occaſion, ſo far as we can perceive, but the intenſity of it only is diminiſhed, by its being diffuſed through a larger quantity of matter. It was, therefore, obvious, that if a quantity of heat is abſorbed, and diſappears in the melting of ice, this would eaſily be perceived when the ice is melted with warm water.

To make this experiment, I firſt froze a quantity of water in the neck of a broken retort, in order to have a maſs of ice of an oblong form.

At the ſame time I heated a quantity of water, nearly equal in weight to the ice, in a very thin globular glaſs, the mouth of which was ſufficiently wide to take in the piece of ice. The water was heated by a ſmall ſpirit of wine lamp applied to the bottom of the glaſs; it was alſo often ſtirred with the end of a feather, and a thermometer hung in it.

While the water was heating, the maſs of ice was taken out of the mould in which it had been formed, and was expoſed to the air of a temperate room, until it was perceived to be beginning to melt over the whole of its ſurface.

I then put a woollen glove on my left hand, and taking hold of the ice, I wiped it quite dry with a linen towel, laid it in the ſcale of a balance on a piece of flannel, and haſtily counterpoiſed it with ſand in the oppoſite ſcale, that I might examine the weight of it afterwards; and I immediately plunged it into the hot water, and extinguiſhed the lamp at the ſame time. The lamp being ſmall, the heat of the water had been increaſing very ſlowly, and had almoſt ceaſed to increaſe; and being examined immediately before I put the ice into it, the temperature was found to be juſt 190 degrees. The ice was all melted in a few ſeconds, and produced a mixture, the temperature of which was 53 degrees.

The weight of the ice, when put into the hot water, was ſeven ounces three drachms and a-half. The weight of the glaſs, with the whole mixture in it, was ſixteen ounces, ſeven drachms, and ſeven grains. The weight of the glaſs alone was nearly one ounce.

In conſidering this experiment, we may overlook quantities leſs than half a drachm, or thirty grains, and reckon the quantities of the different articles, by the number of half-drachms in each.

Thus the weight of the ice was 119 half-drachms.

——— Hot water 135

——— Mixture 254

——— Glaſs alone 16

The melting of the ice was effected, not only by the heat of the hot water, but alſo by that of the glaſs. And, by other experiments, I learned that ſixteen parts of hot glaſs have no more power in heating cold bodies, than eight parts of equally hot water; we may therefore ſubſtitute, in place of the ſixteen half-drachms of warm glaſs, eight half-drachms of warm water, which, added to the above quantity of warm water, make up 143 half-drachms.

The heat of this warm water was 190 degrees, that is 158 hotter than the ice; and if this heat had abated in the mixture only in conſequence of the quantity and coldneſs of the ice, and if nothing had happened when the ice was put in, but merely a communication of this heat, and an equal diſtribution of it through the mixture, the temperature of the mixture ſhould have been 158, viz. the exceſs of heat in the warm water, multiplied by 143, the quantity of the warm matter, and divided by 262, the quantity of the whole, which gives 86.

The mixture ſhould have been 86 degrees warmer than melting ice; but it was found only 21 degrees warmer. Therefore a quantity of heat has diſappeared, which, if it had remained in a ſenſible ſtate, would have made the whole mixture and glaſs warmer by 65 degrees than they were actually found to be. But this quantity of heat

would be ſufficient for increaſing, by 143 degrees, the heat of a quantity of water, equal in weight to the ice alone. It was, however, abſorbed by the ice, without in the leaſt increaſing its ſenſible heat *.

The reſult of this experiment coincides ſufficiently with that of the former; the difference is not greater than what may be expected in ſimilar experiments, and might ariſe from the accident of the central parts of the maſs of ice being colder than the ſurface, by one or two degrees.

I have, in the ſame manner, put a lump of ice into an equal quantity of water, heated to the temperature 176, and the reſult was, that the fluid was no hotter than water juſt ready to freeze. Nay, if a little ſea ſalt be added to the water, and it be heated only to 166 or 170, we ſhall produce a fluid ſenſibly colder than the ice was in the beginning, which has appeared a curious and puzzling thing to thoſe unacquainted with the general fact.

It is, therefore, proved that the phenomena which attend the melting of ice in different circumſtances, are inconſiſtent with the common opinion which was eſtabliſhed upon this ſubject, and that they ſupport the one which I have propoſed.

But ſome perſons may, perhaps, imagine that the heat which thus diſappears does not truly enter into the melting ice, or become combined with that into which it is changed. This heat is, perhaps, entirely extinguiſhed and deſtroyed. As heat has been ſuppoſed by ſome to conſiſt in a rapid motion or tremor of the particles of bodies, or of ſome ſubtile matter that is intermixed with them, thoſe who chooſe to adopt this opinion may imagine that this motion may

* Theſe two experiments, and the reaſoning which accompanies them, were read by me in the Philoſophical Club, or Society of Profeſſors and others in the Univerſity of Glaſgow, in the year 1762, and have been deſcribed and explained in my lectures, there, and in Edinburgh, every year ſince.

meet with friction and reſiſtance in the ice, and that a part of it may thus be deſtroyed, or the moving parts brought to reſt *.

This ſuppoſition, again, as well as the common opinion, will be found inconſiſtent with many phenomena which attend the freezing of water, while theſe phenomena ſtrongly ſupport the idea I have propoſed.

If, for example, we expoſe ſome pure water to the cold air, in a ſharp froſt, and ſet beſide it an equal quantity of equally warm water, with a little ſalt or ſpirit of wine added to it, to prevent it from freezing; we may perceive, by putting a delicate thermometer into each of theſe waters, that they are both cooled by the air at firſt with the ſame celerity, until they are brought down to the freezing point, at the ſame, or very nearly the ſame, moment of time. After this, the progreſs of the ſalted water in cooling will be continued without interruption, until it be as cold as the air to which it is expoſed, which we may ſuppoſe to be ten degrees colder than freezing water. But while this goes on, if we examine what happens in the pure water, when it has been cooled down to the freezing point, we ſhall not find in it the ſame uninterrupted progreſs of cooling: nor will it ſuddenly be changed into ice, which it ought to be, according to the common opinion. It will begin to be changed into ice, but with a very ſlow progreſs, which will require a long time to complete it; and, during all this time, it will continue at the 32d degree of Fahrenheit's ſcale, or 10 degrees warmer than the air to which it is expoſed, until it be totally frozen; after which, it will again begin to be-

* Indeed this ſuppoſition ſeems inſeparable from this mechanical theory of the formal cauſe of heat. It is amuſing to obſerve how the mechanical explainers of the chemical phenomena produce whatever effects they have occaſion for, by their inviſible motions and actions. Here heat is loſt by friction and reſiſtance, &c. The heat is produced, in many chemical ſolutions, by the ſame frictions and violent knockings of the conflicting particles in the act of ſolution—yet the ſame act of ſolution is accompanied with intenſe cold, when ſalt and ſnow are mixed.

EDITOR.

come colder, and proceed without interruption, until it be as cold as the ſurrounding air and the ſalted water.

But, from the beginning of its congelation to the completion of it, although it does not become ſenſibly colder, it is conſtantly communicating heat to the ſurrounding air. A warmer body cannot be in contact with a colder one, without communicating heat to it. We may eaſily ſatisfy ourſelves, that the water, while congealing, is continually imparting heat to the ſurrounding air; for, if we ſuſpend a delicate thermometer immediately above the water, we ſhall find the inſtrument affected by an aſcending ſtream of air, leſs cold than the air around. While the water is congealing, therefore, it is conſtantly imparting heat to the air, without becoming colder itſelf, and this heat muſt be that which had been abſorbed and concealed in it, the laſt time it became water, ſince now, in loſing its fluidity again, this heat comes out of it, although it did not appear immediately before in a ſenſible ſtate *.

In the above deſcribed common proceſs of freezing water, the extrication and emergence of the latent heat, if I may be allowed to uſe theſe terms, is performed by ſuch minute ſteps, or rather with ſuch a ſmooth progreſs, that many may find difficulty in apprehending it; but I ſhall now mention another example, in which this extrication of the concealed heat becomes manifeſt and ſtriking.

This example is an experiment, firſt made by Fahrenheit, but ſince repeated and confirmed by many others.

He wiſhed to freeze water from which the air had been carefully extracted. This water was contained in ſmall glaſs globes, about one-

* Dr. Black made this experiment alſo, and many regiſters of it are to be found among his memorandums: But, in our temperate climate, it is ſeldom that a freezing cold continues with ſufficient ſteadineſs for admitting a ſimple calculation. He made many experiments on the congelation of other ſubſtances, in more uſual and ſteady temperatures: But, in this elementary courſe, intended for the inſtruction of the moſt uninformed hearer, he choſe to confine his proofs to the moſt familiar obſervations.

EDITOR.

third filled, and accurately cloſed, to prevent the return of the air into them. Theſe globes were expoſed to the air in froſty weather, and remained ſo long expoſed, that he had reaſon to be ſatisfied that they were cooled down to the degree of the air, which was ſix or ſeven degrees below the freezing point. The water, however, ſtill remained fluid, ſo long as the glaſſes were left undiſturbed, but, on being taken up and ſhaken a little, a ſudden congelation was inſtantly ſeen.

It has ſince been found, by the trials of others, that the experiment will ſucceed, although the water be not deprived of its air, and that the circumſtances the moſt eſſentially neceſſary are, that it be contained in veſſels of a ſmall ſize, and preſerved carefully from the leaſt diſturbance. The veſſels, therefore, ought to be covered with paper, or otherwiſe, to prevent ſlight motions of the air from affecting the ſurface of the water *. In theſe circumſtances, it may be cooled to ſix, or ſeven, or eight degrees below the freezing point, without being frozen; but, if it be then diſturbed, there is a ſudden congelation, not of the whole, but of a ſmall part only, which is formed into feathers of ice, traverſing the water, in every direction, and forming a ſpongy contexture of ice, which contains the water in its vacuities, ſo as to give to the whole the appearance of being frozen. But the moſt remarkable fact is, that while this happens, (and it happens in a moment of time) this mixture of ice and water ſuddenly becomes warmer, and makes a thermometer, immerſed in it, riſe to the freezing point.

Nothing can be more inconſiſtent with the old opinion concerning the cauſe of congelation than the phenomena of this experiment. It ſhews that the loſs of a little more heat, after the water is cooled down to the freezing point, is not the moſt neceſſary and inſeparable cauſe of its congelation, ſince the water is cooled 6, 7, or 8 degrees below that point, without being congealed.

Some may, perhaps, imagine that the water remains fluid in conſequence only of ſome ſtate of arrangement and connection between its

* *Mairan ſur la Glace.*

particles, which is favourable or neceſſary to its fluidity, and that this ſtate of arrangement and connection continues in this over-cooled water, in conſequence of its being carefully preſerved from every diſturbance.

Although this be only a ſuppoſition, let us admit it for a moment, and try how it will agree with the facts.

If this ſuppoſition were true, then, by diſturbing ſuch over-cooled water, we ſhould ſuddenly bring on a total congelation of it, and it ſhould continue as cold as it was immediately before; but neither the one nor the other of theſe events takes place. A ſmall portion only of the water is thus ſuddenly frozen, and it does not continue equally cold; a quantity of heat ſuddenly appears in it, which was not perceptible immediately before: And this heat, being diffuſed in a moment through the whole mixture of water and newly formed ice, raiſes the temperature ſo high, that no more of the water can be frozen, until more of its heat be taken away.

But theſe phenomena, though inconſiſtent with ſuch a ſuppoſition, and with the old opinion of the manner in which heat produces fluidity, ſtrongly ſupport the one which I have propoſed, and can all be explained by it without difficulty. They actually ſhew the extrication from the freezing water of that quantity of heat which is concealed in its compoſition ſo long as it retains the form of a fluid. This experiment ſhews, that when water is cooled in a ſtate of perfect reſt, in a ſmall veſſel, it is diſpoſed to retain this concealed heat, which I have been uſed to call its latent heat, a little more ſtrongly than in ordinary circumſtances. In common circumſtances, the water retains the whole of this heat, until it be cooled to the 32d degree of Fahrenheit, or a very little lower. If, in ordinary circumſtances, we attempt to make it colder, we may perhaps ſucceed in making it ſtill colder by one degree or two, but no more, for then the latent heat begins to be extricated from a ſmall part of the water, and to appear in the form of ſenſible heat, that ſmall portion of the water which loſes it, aſſuming con-

ſequently the form of ice. Thus, the latent heat of the reſt of the water continues afterwards to be extricated ſlowly and inperceptibly, in proportion as we abſtract the ſenſible heat into which it is changed, until the whole of the water is frozen, or has loſt the whole of its latent heat.

But, when the water is cooled in the circumſtances of the above mentioned experiment, it retains its latent heat and fluidity longer, or until its ſenſible heat be diminiſhed to ſeven or eight degrees below froſt; and, when it is in this ſtate, if it be agitated, or ſuddenly diſturbed by the impulſe of the air, or the falling into it of a little bit of ice, or other ſuch matter, this occaſions the extrication of a part of the latent heat, which now becomes ſenſible heat, and that part of the water which thus loſes its latent heat is at the ſame time changed into ice. But the heat thus extricated at once being in greater quantity than what is extricated in any one moment in the common proceſs of congelation, it is more conſpicuous, by ſuddenly increaſing very remarkably the ſenſible heat of the materials, and limiting the quantity of the ice that is thus ſuddenly formed.

This ſurpriſing experiment, therefore, which formerly appeared ſo ſtrange and unaccountable, can thus be explained. When this experiment is made with great care, in a veſſel inacceſſible to the external air, and in a place where it is not diſturbed by the tremulous motion occaſioned by walking on the floor, or the rattling of heavy carriages, it is poſſible to cool the water even 10 degrees below 32°. If the water be now touched, ever ſo gently, with a ſlender ſpicula of ice, formed by cryſtallization, or a flake of dry ſnow, it inſtantly ſhoots into beautiful ſpiculæ, which rapidly form and branch out in all directions, and the thermometer, which had been left in it, riſes ſlowly to the 32d degree.

But farther, by the ſame principles I alſo explained ſome other remarkable facts relating to cold, of which no explanation had before

been attempted. The facts I mean, are thoſe experiments in which intenſe degrees of cold are produced by mixture.

The mixtures moſt effectual for this purpoſe are thoſe of ice and ſnow with different ſalts; and it always happens, that while the cold is produced, the ſalt and ſnow diſſolve together into a liquid. This liquefaction of the ſalt and ſnow is abſolutely neceſſary to the production of the cold. In order to explain this phenomenon, we need only to recollect, that the liquefaction, which ſuddenly happens, requires, as on other occaſions, that a quantity of heat be abſorbed, and combined, in a latent ſtate, with the liquefied matter; that the mixture cannot aſſume the form of a fluid without abſorbing into it a quantity of heat, or of the matter of heat; and that this heat becomes latent in the compoſition of the fluid that is produced, being combined with the matter of the fluid more cloſely and intimately than heat that is in its ordinary ſenſible and moveable ſtate. This requiſite heat can be had only from the materials. A part, therefore, of the ſenſible heat, which was in the materials before they were mixed, ſuddenly aſſumes that form, and diſappears, ſo as to be no longer perceptible by the feeling of heat, or by means of the thermometer. The mixture, therefore, appears much colder than the materials were immediately before. Moreover, we ſee in this example the converſe of what happens in Fahrenheit's experiments with over-cooled water. In theſe cold mixtures we make a quantity of ice melt ſuddenly, without adding heat to it; and this liquefaction occaſions a great degree of cold to be inſtantly produced, by the converſion of a quantity of ſenſible heat into latent heat. In Fahrenheit's experiment, we occaſion a quantity of water to freeze ſuddenly, by diſturbing it only, and without taking heat from it. A quantity of heat ſuddenly appears in it, or is ſuddenly changed from a latent to a perceptible ſtate.

To underſtand better the power of theſe mixtures in producing cold, and to what extent they can produce it, we muſt recollect, that ſalt added to water increaſes conſiderably its diſpoſition to fluidity, but

increaſes it only to a certain degree. In a ſtrong brine of common ſalt, for example, the diſpoſition of the water to fluidity is ſo much greater than in pure water, that the brine will bear to be cooled down to the beginning of Fahrenheit's ſcale, or even a few degrees lower, without ſhewing any beginning of congelation. But if, by expoſing to the action of a ſufficiently powerful cooling cauſe, we cool it a little more, for example, to five or ſix degrees below Zero, then the chemical attraction of the ſalt and water for one another begins to be overcome by the coheſive attraction of each of theſe ſubſtances; a ſmall part of the ſalt concretes into grains like ſand, and falls to the bottom, and a ſmall part of the water freezes. This is accompanied by the extrication and emergence of a proportional quantity of latent heat, or the converſion of it into ſenſible heat, which increaſes the ſenſible heat of the brine a little, and puts a ſtop to the farther ſeparation of the ſalt and congelation of the water, until this ſmall addition of ſenſible heat be taken away again by the cooling cauſe. But as ſoon as it is taken away, a little more of the ſalt concretes, and a little more of the water freezes. All which repeatedly happens, until at laſt, the whole of the latent heat having been gradually extricated, the ſalt will be found at the bottom, all in ſmall grains, and the water over it all congealed.

From this we can explain the peculiar phenomena of ſome of theſe experiments. For example, it is remarkable of the mixture of ice and common ſalt, that it never produces a cold greater than Zero, or a few degrees below Zero, but that it continues cold to this degree for a conſiderable length of time, during which the ſalt and ſnow are gradually melted. Now it muſt neceſſarily happen, that as ſoon as they begin to melt, ſo much of their heat will be converted into latent heat, by the melting of a ſmall part of theſe materials, that their remaining ſenſible heat will be greatly diminiſhed. It will be only ſufficient for heating them to the degree Zero on Fahrenheit's ſcale; but, when the remaining ſolid ice and ſalt are cooled to this degree,

they cannot act further on one another by chemical attraction. The coheſive attraction of each is become too ſtrong to admit of this; they cannot act further, until a little more heat be communicated to them from the ſurrounding bodies. As ſoon as this happens, they again act, and a ſmall part of them melts, which again brings down their ſenſible heat to the low level; therefore they muſt neceſſarily unite together, and melt, with a very ſlow progreſs, regulated by the gradual communication of heat from the ſurrounding bodies, which heat, as faſt as it is communicated, is changed into latent heat, and the ſenſible heat of the mixture muſt continue at the ſame low degree, ſo long as any conſiderable part of the ice and ſalt remains unmelted. Some other ſalts increaſe, to a ſtill greater degree, the diſpoſition of water to fluidity, and will therefore produce a much greater degree of cold in this way.

It will not be unacceptable, I preſume, on this occaſion, to mention the moſt ſucceſsful and convenient proceſſes for producing great diminutions of heat, or, as it is more commonly expreſſed, for producing intenſe colds. This is not a matter of mere curioſity, but it is frequently the means of philoſophical inveſtigation; and ſome very intereſting facts have been diſcovered in this way.

I think you are now ſufficiently aware, that, ſince fluidity is the conſequence of a certain combination of calorific matter with the ſubſtance of ſolid bodies, it muſt follow, that if any ſubſtances are induced, by their chemical properties, to act on each other, ſo as mutually to diſſolve and become fluid; and if there be no external ſource, from which the neceſſary latent or fluid-making heat can be obtained, it muſt be furniſhed by the materials themſelves; that is, ſome of their ſenſible heat muſt diſappear,—their temperature, indicated by the thermometer, muſt be lowered: And this muſt happen in a degree ſo much the more remarkable, as there is more of the ſolid matters rendered fluid, and alſo, in proportion as the ſubſtances, ſo liquefied, require a greater quantity of latent heat for this purpoſe. An ounce of

one ſubſtance may thus abſorb as much as two ounces of another ſubſtance.

You can eaſily ſee further, and I have already mentioned ſome examples of it, that, whatever are the ſubſtances employed, there muſt be a limit to the diminution of temperature. If the liquid mixture congeals at any particular temperature, at 40° for example, it is impoſſible to produce a greater cold by mixing thoſe ſubſtances.

Theſe obſervations, therefore, ſuggeſt certain maxims or rules for the powerful operation of chemical mixtures for producing intenſe colds. We muſt chooſe ſubſtances whoſe chemical action induces them ſtrongly to diſſolve each other in a liquid form. They ſhould alſo be ſuch as require a great quantity of latent heat: And they muſt be ſuch whoſe mixture will bear a very great diminution of temperature before it freezes. I cannot ſay that theſe circumſtances have been made the ſubject of particular inquiry, ſo as to enable us, *a priori*, to make the moſt effective ſelecton of materials. It has generally been by random and numerous trials.

There is another maxim which may be deduced from the knowledge we have acquired. When a certain ſubſtance requires a determined quantity of latent heat for its appearance in a liquid form, it is plain, that when other circumſtances are the ſame, it will produce, not a certain determinate temperature, but a certain determinate diminution of temperature.

Of this conſequence we can make a moſt effective uſe. Suppoſe that a certain mixture produces a diminution of 20 degrees, and that it is made in the temperature of 50, it will acquire the temperature 30 degrees, and no lower. But we can employ this temperature to cool another quantity of the ſame ingredients ſeparately to the temperature of 30 degrees. Let theſe ingredients be mixed: we ſhall then produce the temperature 10 degrees; and if this be employed in the ſame manner, to cool another parcel of the ſame ingredients, their mixture will produce the temperature −10°. By the repe-

tition of this proceſs, we ſhall at laſt reach the utmoſt cold that is producible by thoſe ingredients.

I have juſt to obſerve further, that as, during this proceſs, the external air, muſt be continually imparting heat to our mixtures, and this with rapidity, when the cold already produced is very intenſe, it is neceſſary to cut off this ſupply as much as poſſible. The experiments ſhould be made when the air is very cold. The veſſels ſhould be ſet in other veſſels, made of bad conductors of heat; and the interval ſhould be occupied by furs, or down, or other ſpongy matters.

I thought it beſt to give you theſe general principles and maxims, deduced from the knowledge we have acquired, that you may perhaps fall upon more effective methods than any I can relate or deſcribe.

The ſubſtances generally employed have been ſalts diſſolved in water. None has been found more efficacious than ſal ammoniac. If three parts of water be added to one part of this ſalt, the liquefaction lowers the temperature fully 20 degrees of Fahrenheit. We can therefore freeze a little cold water when the air is at 48°, the temperature of ordinary fountains in this country. Two ſuch operations would freeze a little water, even in our ordinary ſummer heat.

A much ſhorter and effective proceſs was diſcovered by Mr. Walker, apothecary, in Oxford. He mixed 11 ounces of ſal ammoniac, 10 of ſaltpetre, and 16 of Glauber's ſalt. He then poured on this mixture of dry ſalts 32 ounces of water, ſtirring the mixture, to accelerate their ſolution. Thus he produced at once a diminution of 48 degrees of Fahrenheit. The experiment being made when the temperature of the materials was 65°, it was changed to 17°, which is 15 degrees below froſt.

He produced ſtill greater colds, by mixing ſome ſalts with acids. Three parts of aquafortis mixed with 12 of Glauber's ſalt, ſunk the thermometer 52 degrees: and then, adding ſix parts of ſal ammoniac, he ſunk it eight degrees further. He froze quickſilver, by repeating

this proceſs. The vitriolic acid alſo, diluted with an equal weight of water, produced intenſe cold, by diſſolving cryſtallized Glauber's ſalt. It ſunk the thermometer (then very low) 46 degrees.

Dr. Guthrie writes me from Peterſburgh, that Mr. Lowitz, in December 1792, when the thermometer was at 60 degrees of Fahrenheit, froze completely four pounds of quickſilver, by the mixture of dry ſnow with cryſtallized cauſtic potaſh. As the preparation of this ſalt is very troubleſome, and as it corrodes every thing it touches, and is dangerous for the operator, he tried another ſalt, called fixed-ammoniac, which is the reſiduum in the manufacture of cauſtic volatile alkali, and is very cheap. When this is evaporated, till a drop of it on a plate fixes immediately, it is then allowed to cool and grow ſolid. It attracts water with greedineſs, and muſt be kept from deliquiating, in well corked bottles. This ſalt, mixed with dry ſnow, is very nearly as powerful as the potaſh, and alſo froze the quickſilver. It is, perhaps of all others, the moſt generally convenient: it is cheap, and eaſily preſerved fit for this uſe; and when uſed, it will ſerve again as well as ever, by evaporating the water produced in the experiment. Pounded ice may be uſed inſtead of ſnow in ſummer; but it muſt be inferior in efficacy, becauſe there adheres to it ſome water already provided in latent heat. Mr. Lowitz found that both of theſe ſalts were twice as powerful in producing cold as ſmoking nitrous acid, which produces the depreſſion 41 degrees. He found that theſe ſalts produced about 80 degrees. If theſe numbers have been accurately reported to me, this method of Mr. Lowitz's is the moſt efficacious that I know *.

As ſoon as the ſolution ceaſes, the cold begins to abate, by the influence of external warmth. The proceſs muſt therefore be conducted expeditiouſly, and with the defences mentioned above. Ice and common ſalt, melting ſlowly, produce a cold of conſiderable continuance and intenſity. But I do not imagine that it can be lower than about

* I find the ſame numbers in the account given by Crell in his *Chem. Annals*, 1793. I. 352. EDITOR.

—5° of Fahrenheit, becauſe, about that temperature, the ſalt, being no longer attracted by the water, begins to cryſtallize and give out its latent heat. I am uncertain what may be the limits of thoſe other mixtures.

Thus I have given the facts relating to the melting of ice and freezing of water, connecting them together, ſo as to lead to the ſame concluſion; and I have communicated the notions and reaſonings which I founded on thoſe facts. I muſt now add, that the ſame notions and reaſonings may be applied to the liquefaction of many, and, probably, of all other bodies by heat: At leaſt, a great number exhibit appearances when they are liquefied, and when they return from the fluid to the ſolid ſtate, very ſimilar to thoſe exhibited by ice or by water, when undergoing theſe changes.

The late ingenious Dr. Irvin, who was my pupil at Glaſgow, and afterwards my ſucceſſor in the chemical chair, making ſome experiments, at my deſire, on the melting and congealing of ſpermaceti, and bees-wax, and ſome other ſubſtances, found that the ſpermaceti in melting, abſorbed a quantity of heat, which, though it did not make it ſenſibly warmer, but only fluid, would have been ſufficient for making melted ſpermaceti warmer by 141 or 148 degrees.

Bees wax abſorbed a quantity equal to what is ſufficient for making melted bees wax warmer by 175 degrees. And from experiments with tin, he found reaſon to conclude, that this metal, while melting, abſorbs a quantity of heat equal to what might add 500 degrees of heat to it in its ſolid ſtate.

It appears alſo from Dr. Guthrie's experiments on the congelation of quickſilver *, that it abſorbs a great quantity of heat, while it is changed by heat from a ſolid to a fluid. A cylinder of congealed quickſilver, adhering to the bulb of a thermometer, being carried into a very warm room, very ſoon began to melt; but it required a conſi-

* Phil. Tranſ. 1786.

derable time to melt it completely, the thermometer involved in it pointing all the while to the —40th degree of Fahrenheit. As the quickſilver all this time was ſo very much colder than the air around, it muſt have received heat in a great quantity from that air.

Appearances alſo occur in the refining of copper and of ſilver, and the melting and moulding of phoſphorus, which not only ſhew that thoſe ſubſtances, when melted, contain a quantity of latent heat, but that, in certain circumſtances, they can, like water, retain this latent heat until they are cooled conſiderably below their congealing point. Another opportunity will be taken of mentioning ſome of theſe facts, which are very curious, but cannot be fully explained without a previous knowledge of the peculiar properties of thoſe ſubſtances. I am perſuaded alſo that the great and ſudden contractions of the quickſilver in Profeſſor Braun's thermometers, depended on the ſame circumſtances. The quickſilver was cooled beyond the degree ſufficient for its congelation, yet ſtill retained its latent heat and fluidity, but being then diſturbed, was made to congeal, at leaſt in part, and the congelation of it is attended with extraordinary contraction *.

There is reaſon to think, that not only the fluidity of bodies, but that even the ſoftneſs of ſuch as are ſoftened and rendered plaſtic by

* Dr. Black, in ſeveral courſes of his lectures, extended this doctrine much farther, namely, to the caſes of ſolution of ſalts in water, and ſeveral others, when ſolid bodies are rendered fluid by the action of proper fluids upon them; as alſo to ſome caſes where fluid bodies become ſolid by chemical action, as in the churning of butter, the formation of cinnabar, &c. Some of theſe are exceedingly curious, and greatly puzzled the philoſophers. Thus, if a ſaturated ſolution of Glauber's ſalt, in hot water, be allowed to cool in a place where it ſuffers no agitation or tremor, it will frequently grow cold without cryſtallizing. If it be now ſhaken, it cryſtallizes ſuddenly, and the veſſel becomes very warm, and even hot. In the combination of mercury with ſulphur, by the aſſiſtance of heat to melt the ſulphur, when the true chemical union takes place in a conſiderable quantity at once, it becomes ſtiff on a ſudden, and heat emerges ſufficient for kindling the ſuperfluous ſulphur.

EDITOR.

heat, depends on a quantity of heat combined with them in the form of latent heat. In the experiments made by Dr. Irvin on the congelation of bees wax, it was quite plain, that after the wax had loſt its fluidity, and while, from its ſoft ſtate, it ſlowly became harder, till it acquired its greateſt hardneſs, a quantity of latent heat was gradually extricated, and, aſſuming the form of ſenſible heat, kept up the heat of the cooling wax much longer, and occaſioned its abating much more ſlowly than it would otherwiſe have done. The doctor was ſurpriſed at it, and was at a loſs to account for it, until I pointed out to him the cauſe that appeared the moſt probable, which was, that the ſoft ſtate of wax is produced by a certain quantity of latent heat in it, though a much leſs quantity than that neceſſary to give it fluidity.

On account of this, and ſome other facts, I began to ſuſpect, that the malleability and ductility of metals depend on the ſame cauſe; and, after mature reflection, I am perſuaded that this is the caſe. While metals are hammered, and extended under the hammer, they become warm, and in ſome caſes very hot; but at the ſame time they become very rigid, and are no longer malleable. If we attempt to beat them out farther, they are ſure to crack at their edges, not having the ſame toughneſs and ſoftneſs as before. The only way now to reſtore that toughneſs is by annealing them, that is, by making them hot in the fire, and allowing them to cool. This being done, they are found malleable again, but liable to loſe this malleability by a ſecond hammering, and they need the introduction of heat into them again, if we deſire to hammer them more.

The following experiment gave me a proof of that. In order to anneal them, the ſingle circumſtance of their being heated is not ſufficient, but it is neceſſary that the heat be communicated to them from other matter. I deſired a ſmith to make iron red hot by hammering it. He very ſoon did it by hammering the extremity of a rod of iron properly prepared. It aſſumed a moderate, or dull red heat under the action of the hammer. I deſired him to let it cool, and

then hammer it again, to make it red hot a ſecond time. He told me this could not be done without firſt annealing it, or ſoftening it *in the fire;* and that if he ſhould attempt to hammer it again without this preparation, it would not bear the ſtrokes of the hammer without ſplitting and breaking into pieces, of which he ſatisfied me a little while after. But when the iron was in this brittle ſtate, it needed only to be heated in the fire until it had a dull red heat, ſimilar in appearance to that which had been produced in it by the ſtrokes of the hammer; being then allowed to cool, it was found malleable. It is plain, that if nothing were requiſite for annealing the iron in this experiment, but making it red hot, and allowing it to cool ſlowly, this is already done; for the iron is left red hot by the hammer.

I therefore conſider the metals as ſubſtances, which have the power to retain ſtrongly a certain quantity of latent heat, which gives them their toughneſs and malleability, but I imagine that this heat is driven out of them by the violent agitation, compreſſion, and friction of their parts, in hammering them ſtrongly into another ſhape. Thoſe called the more perfect metals retain this heat with the greateſt force, and retain it in ſome caſes, though extended by ſkilful hammering to an amazing degree. Tough iron, which is a purer metal than ſteel, contains more of it than ſteel does, and ſhews a little more power to retain it; from iron it cannot be expelled but by the ſtrokes of the hammer, or violent compreſſion; from ſteel it can be ſeparated not only by hammering, but alſo by ſudden and violent refrigeration of the ſteel from a red hot ſtate. This happens in the operation called the hardening of ſteel: The ſteel is made red hot in the fire, and then ſuddenly plunged into cold water; thus it is made exceſſively hard, but at the ſame time perfectly inflexible and brittle. We muſt therefore conclude, that this ſudden and violent refrigeration prevents its retaining a due portion of latent heat, which it would have retained, had it been allowed to cool ſlowly and quietly. Iron, when treated in the ſame manner, loſes but very little of its latent heat.

And now I have given, what appears to me, a juſt account of the nature of fluidity and ſoftneſs, conſidered as general effects of heat, and of the manner in which theſe qualities are produced by heat; together with a number of facts belonging to this ſubject, from which it may be perceived, that when properly underſtood, they are connected together, and explain one another, as well as ſome other facts of which no explication had been offered before.

But ſince I have been accuſtomed to communicate this view of the ſubject, attempts have been made to explain ſome of the facts a little differently. It has been imagined, that in order to account for the phenomena, I ſuppoſe a cauſe which is not at all neceſſary, namely, that it is a ſpecial combination of the ſubſtance of bodies with the material cauſe of heat, which, while it continues in this ſtate of combination, I call *latent heat*. This ſuppoſition has been ſaid to be unneceſſary, becauſe the appearances can be explained by another principle, which, it is ſaid, is an uncontrovertible fact, which muſt neceſſarily produce the very appearances that we obſerve.

The alleged fact or principle is this: A body, while in its fluid ſtate, has a greater capacity for heat, or abſorbs more heat in order to riſe one degree in its temperature, than when it is in the ſolid form. Thus, for example, ice, it is ſaid, has leſs capacity for heat than water has. When ice, therefore, is changed into water, a quantity of heat muſt go into this water, without making it ſenſibly warmer; and when water is changed into ice, a quantity of heat muſt come out of the freezing water as faſt as the ice is formed, and this without leaving it ſenſibly colder. The abſorption of heat, therefore, into the melting ice, is not the cauſe of its liquefaction, but rather the conſequence of it; while on the other hand, the extrication of what I call latent heat from the freezing water, is not the cauſe of its becoming ſolid, but the conſequence of it.

This way of conceiving the ſubject occurred firſt to Dr. Irvin. I never had an opportunity to learn the terms in which he ſtated this

opinion. I have here ſtated it in the manner in which I underſtood it from report, and from what Dr. Crawford afterwards publiſhed *.

But to this ſtatement I anſwer, that the alleged fact of diſparity between the capacity of ice and water for heat, may indeed be ſuppoſed to account for the thermometrical phenomena juſt now recited; but the principal fact or phenomenon is not accounted for by it, I mean the change of the ſolid into the fluid. Solidity, we all know, depends on coheſive attraction; but on what cauſe does fluidity depend? Will it be ſaid to depend on the abſence or ceſſation of coheſive attraction? I cannot imagine how coheſive attraction can entirely ceaſe, or be ſuſpended, or, if this ſhould happen, how it ſhould eaſily be again reſtored: Nay, we have evident proof, from the round form of the drops of liquids, that it is not in them entirely ſuſpended, but only weakened to ſuch a great degree, that the ſolid hard body is become a liquid. Now this is ſuch a violent change, that I cannot help thinking it muſt depend on ſome powerful cauſe. It may be ſaid, perhaps, that no other cauſe is neceſſary but the increaſe of ſenſible heat above a certain degree, together with ſome ſingularity in the nature of coheſive attraction; ſuch as that this attraction, though very ſtrong at certain ſmall and imperceptible diſtances, becomes ſuddenly very weak, when we increaſe, beyond a certain limit, the diſ-

* It has already been mentioned, when treating of the unequal diſtribution of heat, how Dr. Irvin was led to attend very minutely to the different capacities of bodies for heat; and his notion of the heat, which appeared when we mix vitriolic acid, in its moſt concentrated ſtate, with water, has alſo been noticed. This heat is frequently ſuch as to be inſupportable by the hand, and ſplits the glaſſes in which the mixtures are made. It is not wonderful, that Dr. Irvin ſhould think this ſufficient to account for the ſlow extrication of 140 degrees of heat. It muſt be added, that the few experiments which this temperate climate allowed him to make, on the comparative heats of ice and water, and the low temperature which he thought was the commencement of abſolute heat, authoriſed him to ſay, that the ſum total of the exceſſes of the ſpecific heat of water, above thoſe of ice, was more than ſufficient to account for all the heat that emerges in freezing, or is abſorbed in melting. EDITOR.

tance of the particles of matter from one another, and this increaſe of diſtance is produced by ſenſible heat.

If this were true, the increaſe and diminution of diſtance by the action of ſenſible heat, ought always invariably to produce, each its appropriated effect, on the power of the coheſive attraction; as water, when its degree of heat is above 32°, is always liquid, ſo when its ſenſible heat is below 32°, it ſhould always be ſolid. But this we know is not always the fact. Water, in ſome circumſtances, can be cooled to 7 or 8 degrees below 32° without being congealed; and many other ſubſtances exhibit the ſame phenomenon, by retaining their fluidity in ſome circumſtances, after their ſenſible heat is diminiſhed conſiderably below their ordinary congealing point. And I now aſk, what is the cauſe which hinders the coheſive attraction from producing its effect, and changing the liquid into a ſolid? When I find by experience, that, upon diſturbing ſuch over-cooled liquids, a quantity of heat is extricated from them, which did not appear immediately before, and that, while this heat is extricated, a proportional part of the liquid congeals; I cannot help conſidering this latent heat which was in it, as having been the cauſe of its protracted fluidity.

We have alſo, in the experiment on iron above deſcribed, a ſtriking example of the difference between ſenſible and latent heat, and of the want of power in the one to produce the effects of the other. When the latent heat of the ſoft iron was extricated by hammering, and changed into ſenſible heat, it had no longer the effect to give toughneſs and malleability to the iron, but the greateſt part of it was communicated to the ſurrounding bodies, as ſenſible heat always is; thus leaving the iron deprived of the circumſtance neceſſary to its toughneſs. Moreover, by no other means can this quality be again reſtored to the iron, but by giving it an opportunity to recover in the fire, and retain, in a ſtate which does not affect the thermometer, this quantity which had been expelled from it. In which proceſs of an-

nealing the iron, it is not neceſſary to heat it to a greater degree of redneſs than that which was produced in it by the eruption of its latent heat.

I am, therefore, ſtill of the ſame opinion that I mentioned to you a little ago upon this ſubject. I conſider fluidity as depending, immediately and inſeparably, on a certain quantity of the matter of heat which is combined with the fluid body, in a particular manner, ſo as not to be communicable to a thermometer, or to other bodies, but capable of being extricated again by other methods, and of re-aſſuming the form of moveable or communicable heat.

We now ſee the reaſon for the ſixth caution given for conducting the experiments for determining the ſpecific capacities of bodies for heat. If they act on each other, ſo as to produce the ſolution, or even the ſoftening, of one of the bodies, heat will be abſorbed; or, if the mere heat of the mixture melts or ſoftens either of them. For the ſame reaſon, we cannot examine the temperature of intenſe heats in Newton's method, when the body examined freezes during its expoſure to the ſtream of cold air; nor can we diſcover it by pouring the melted body into water, if the temperature produced be ſo low that the body freezes. *(See Note 4. at the end of the Volume.)*

SECT. III.—OF VAPOUR AND VAPORISATION.

HAVING conſidered, in ſufficient detail, the appearances which are exhibited when ſolid bodies are melted by heat, and having ſhewn the connection which the different circumſtances accompanying this change have with the general operation of heat, we now proceed to treat of another, and more remarkable change, produced by this great agent in nature; I mean the converſion of bodies into an elaſtic vapour, that is, into a fluid extremely rare, light, and expanſive, like air, capable, like it, of being eaſily reduced into leſs

ſpace, by external preſſure, and reſiſting, like it, the force which thus compreſſes it. That you may conceive this more diſtinctly, I ſhall direct your attention chiefly to this operation of heat as it affects fluid bodies, and, taking water as the moſt familiar inſtance, I ſhall deſcribe the manner in which it ſuffers this change. Suppoſe a ſingle tea-ſpoonful of water to be put into a globe of glaſs, or metal, capable of holding ſome gallons, and ſuppoſe this veſſel exhauſted of air. If we apply heat to the globe, we ſhall perceive the water gradually to diſappear, ſo that preſently the globe ſhall ſeem empty. But the fact is, that it is completely filled with the water, now exiſting in the form of a perfectly tranſparent vapour; for, if the heat be ſtill farther increaſed, the expanſive force of what now fills the globe will alſo increaſe to ſuch a degree, as even to burſt it.

Upon this change of water from its ordinary ſtate into that of a rare expanſive fluid, depend the effects of what are called candle-bombs. Theſe are hollow ſpherules of glaſs, formed in the lamp, on the end of ſlender tubes. A little bit of the tube is left adhering to the ball. By holding the ball in the flame of a candle, part of the air is driven out by expanſion. The end of the tube is then dipped into water, and, as the air in the ball collapſes again by cold, the water is forced into it by the preſſure of the atmoſphere, till it is perhaps half full. The tube is then ſealed hermetically. If this little ball be ſtuck into the wick of a burning candle, the water is converted into elaſtic vapour, which burſts the glaſs with a loud crack. As the vapour expands in all directions, the candle is blown out, and the wick is commonly beaten down, as if by a blow with a hammer.

In like manner, the æolipile affords a very plain proof of the great rarity and elaſticity of the vapour into which water is converted by heat. The æolipile is a hollow ball of copper, having no opening but a narrow pipe, which is bent at the top, nearly at right angles. It is partly filled with water, in the ſame manner as the candle-bomb. If it be now ſet on burning coals, with its pipe pointing into the

midſt of the fuel, we ſhall ſoon perceive a violent blaſt proceed from it, which, when properly managed, will blow up the fire like a pair of bellows. This will continue till the water is all expended. We ſhall afterwards learn that it blows up the fire, chiefly, becauſe the vapour of the water drags along with it the ſurrounding air, and forces it through the interſtices of the fuel.

It is partly owing to this converſion of water into elaſtic vapours that it is ſo dangerous to allow it to come near hot oils. The boiling of lintſeed oil, for example, is always done without doors, to prevent accidents; and we are obliged to chooſe a dry day, becauſe a drop of rain falling among the oil would ſink down through it, and would be ſuddenly converted, by the exceſſive heat of the oil, into a great bulk of expanſive vapour, which would daſh the oil out of the veſſel into the fire. I ſaid that this vaporiſation of the water is *partly* the cauſe of ſuch accidents, becauſe it is known, of late years, that the water is by this means converted into another expanſive fluid, much more bulky and elaſtic than the mere hot vapour of water. Of this you will learn more afterwards. It is the water which all oils contain, in a ſmall quantity, that is the cauſe of their firſt frothy boiling, after which they remain quiet, till raiſed to a vaſtly higher temperature. In like manner, it is dangerous to admit water, or even dampneſs, into the moulds in which metals are caſt. In the caſting of iron, they form the moulds of ſand, which being very porous, allow the humidity to eſcape without any violence. When copper is melted in very large maſſes, it contains a prodigious quantity, both of ſenſible and latent heat, which gives it a great power of converting water into expanſive vapour. It has ſometimes happened that a perſon, by careleſsly ſpitting in a copper foundery, has occaſioned an exploſion that deſtroyed the whole building.

The bulk into which water expands, by the ordinary boiling heat, producing a ſteam equally elaſtic with common air, is very great. It has been, however, much over-rated by ſome very reſpectable authors.

Dr. Deſaguiliers, on the authority of Mr. Nieuwentyt, and ſome of his own experiments, ſays that it expands into 14000 times its natural bulk. But, by my own experiments, and thoſe of my friend Mr. Watt, whoſe intereſt in this matter made him uncommonly accurate, its greateſt expanſion in theſe circumſtances, does not exceed 1800 times its former bulk.

This change is produced, like fluidity, in conſequence of the heat of the body being increaſed beyond a certain temperature. This is found to be different for almoſt every different kind of matter *. Like fluidity, alſo, this temperature muſt be continued, in order that the vaporous form may remain. If the heat be again diminiſhed, the vaporous form is deſtroyed, the vapour loſes its elaſticity, and again collapſes into that form from which it was produced. You muſt all have obſerved this in the caſe of water, the ſteam of which is condenſed into water again, whenever it ſtrikes any maſs of cold matter, ſuch as a lump of iron.

In general, therefore, the converſion of any particular kind of matter into vapour, depends on its being heated above a certain degree, and this vapour collapſes again, as ſoon as its heat is diminiſhed below that degree. This degree, or vaporific point of heat, is found (with reſpect to moſt bodies) to be higher than that which is neceſſary for giving them fluidity. Almoſt every particular kind of matter requires a different temperature for this purpoſe.

Hence the chemical terms of VOLATILE and FIXED. Thoſe bodies which are moſt eaſily converted into vapour are generally called volatile, and this diſpoſition to vaporiſation is called VOLATILITY. Thoſe bodies, on the other hand, are called FIXED, which are with greater difficulty converted into vapour, or require a higher temperature for

* The earlieſt notice that I can meet with of this obſervation, is that by Dr. Hooke, who propoſed the temperature of boiling water, as a fixed point in the ſcale of the thermometer. This was in 1684. *See Birch's Hiſt. of the Royal Society*, vol. iv.

EDITOR.

this converſion. Sometimes, indeed, the term fixed, or fixedneſs, has been underſtood to mean a total want of volatility, or capacity of being converted into vapour by any degree of heat. But this is not the true meaning, and it is doubtful whether any body in nature is unſuſceptible of vaporiſation by heat. Theſe terms, therefore, are only terms of compariſon, and a fixed ſalt is not a ſalt incapable of vaporiſation by heat, but one that requires a high temperature for its converſion.

I have repreſented the degree of heat at which any particular kind of matter begins to be converted into elaſtic vapour, as ſteady or determinate, in the ſame manner as the degree which gives it fluidity. I muſt now reſtrict this notion, ſo far as to obſerve that it is ſteady, only when bodies are heated under the ſame circumſtances. But there are ſome circumſtances, which, when they are varied, occaſion very great differences in the vaporific temperature of every kind of matter. This point, therefore, is not ſo unchangeable as the melting point. So far as experience goes, the melting point of each kind of matter is always the ſame; nothing but a certain invariable intenſity of heat can give fluidity to a particular ſolid ſubſtance, nor can any variation of circumſtances hinder that ſolid from becoming fluid, when raiſed to the melting point, and a proper quantity of heat is communicated to it. But the change of a body into vapour is effected by preſſure, as well as by heat. In proportion as the external preſſure upon its ſurface is greater, it endures a greater heat, without aſſuming the vaporous form, than it would otherwiſe do; and thus we may ſay, that vaporiſation is retarded or oppoſed by external preſſure.

This was firſt obſerved, I think, by Boyle, when making experiments with his air pump. Some time after this, Fahrenheit, when graduating his thermometers, obſerved that the degree of heat which produces elaſtic vapour from water, and is called its BOILING POINT, was liable to a little variation at different times, even when examined with the ſame thermometer. Attending to all the circumſtances that

accompanied theſe variations, he found that they always correſponded with the changes of the weight, or preſſure of the atmoſphere. In future, therefore, he always attended to the height of the barometer, when he marked the boiling point upon his thermometers. The thing was ſoon very well known, and the variations of the boiling point, correſponding to the heights of the mercury in the barometer, were noted with preciſion by thoſe who were intereſted in this ſubject. As the changes in the barometer are not very great, ſo the variations in the temperature of boiling water are but inconſiderable, never exceeding two or three degrees in theſe countries. (See Dr. Martin's *Eſſays on Heat*—Sir Geo. Shuckburgh *on the Variations of the Boiling Point*—*Phil. Tranſ.* vol. 69.)

The variations of the boiling point of fluids, by ſtill greater changes of external preſſure, have been more particularly conſidered by different writers, and machines have been contrived where their effects have been much more conſpicuous. The moſt remarkable of theſe is the veſſel called a DIGESTER, the invention of Mr. Papin, a very ingenious French phyſician, reſiding in London. Papin's digeſter is a copper veſſel, generally cylindrical, having a lid nicely fitted to it, and kept faſt by ſcrews. It is uſual to interpoſe a ſlip of ſoft paper between the digeſter and its lid, to prevent the elaſtic vapour from forcing its way out. If this veſſel be half filled with water, and the lid ſcrewed tight down, and if it be then ſet upon burning coals, a portion of the water is ſoon converted into STEAM (this is the name given to the elaſtic vapour of boiling water.) This converſion begins in the boiling temperature of the water; but the elaſtic vapour being confined, it preſſes on the ſurface of the water, and thus prevents the converſion of any more of it into ſteam. But the continued application of heat raiſes the temperature of the water above its ordinary boiling point, and occaſions the converſion of a ſmall portion more of it into vapour, whoſe elaſticity is at leaſt equal to that of the ſteam already exiſting. And thus the production of ſteam is

again ſtopped till the heat be raiſed ſtill higher, and the elaſticity of the ſteam be ſtill farther increaſed. We are not very well informed concerning this internal procedure, nor very certain whether any ſecond, or ſubſequent, portion of water is thus converted into ſteam, by the increaſe of heat. This muſt depend upon the rate at which the increaſe of heat augments the elaſticity of the ſteam already produced.

We know, however, that by the confinement of the ſteam, and the conſequent increaſe of the preſſure upon the ſurface of the water, it may be made to endure very conſiderable degrees of heat. The ingenious inventor employed it chiefly for increaſing the diſſolving power and general activity of water, and watery fluids, in various operations of cookery, pharmacy, and other chemical arts. He thus enabled water completely to diſſolve horn, tortoiſe-ſhell, cartilage, and even bone, into a jelly. He made many extracts, which could not be obtained in any other manner. Theſe effects induced him to call his apparatus a digeſter. Mr. Muſchenbroeck found, that a heat might be produced in the water contained in the digeſter, ſufficient for melting bits of tin and lead, that were hanging by wires in the midſt of it. Theſe are very hazardous experiments, and terrible accidents have ſometimes happened, by the burſting of the digeſter.

This variation in the temperature, neceſſary for the converſion of bodies into elaſtic vapour, is not peculiar to water, but obtains univerſally. Seeing, however, that the preſſure of the atmoſphere does not vary very conſiderably in any one place, where obſervations are made, the vaporific temperature of water and other bodies, when not ſubjected to any artificial preſſure, is pretty ſteady. Theſe are the temperatures which are generally conſidered and mentioned as their natural vaporific points. No doubt, in one ſenſe, they are ſo, being the temperatures at which ſuch bodies are converted into vapour, in the ordinary courſe of obſervation. But they are by no means the heats at which ſuch bodies would begin to aſſume the vaporous form, if left to their own natural diſpoſition alone, and expoſed to no other action but that of heat. But, under the preſſure of the atmoſphere, every

body is forced to receive much more heat than it would otherwiſe endure, without aſſuming the form of vapour. Some experiments, made by Mr. Robiſon, upon water, ſpirit of wine, mixtures of ſpirits and water, and ſulphuric acid, ſhew that the difference between the temperatures at which they boil, *in vacuo*, and in the open air, is nearly the ſame in all, namely, 120 degrees of Fahrenheit's ſcale *.

As the preſſure of the atmoſphere does not commonly affect our ſenſes, it does not engage our attention, and we bear it without inconvenience, or rather our conſtitution is ſuch, that we cannot exiſt without it. But we muſt not overlook this when treating the ſubject of vaporiſation. We muſt remember that we live under an ocean of air, which covers the whole globe, to the height of about 50 miles, and which has a very ſenſible weight. Thus, we find that a large bottle is heavier, when filled with air, than after it has been extracted from it. A cubic foot of air weighs about $1\frac{1}{8}$ ounce Troy. Therefore, when we boil a fluid at the top of a high mountain, we do it under a ſmaller preſſure than upon the ſea-ſhore. We muſt attend to this in trying the boiling heats of different fluids, at different elevations above the ſea.

Let us now attend to ſome other differences of circumſtances and conditions, under which vapour may be produced. When a ſmall quantity of any fluid, a drop of water, for inſtance, is heated very quickly, as when we throw it upon a piece of red hot iron, it is ſoon heated up to its vaporific point, becauſe every part of this quantity of matter receives the heat nearly in an equal manner, and, almoſt at the ſame inſtant, arrives at that temperature

* Theſe experiments were made by me in 1764, by the help of a very indifferent air-pump. I have repeated them ſince, in air ſtill more rarefied, and ſtill have reaſon to think that the difference of temperature is very nearly the ſame in all of them, but 22 or perhaps 25 degrees more than what I then found.

EDITOR.

which it cannot paſs without being converted into vapour. Hence it happens that the drops run about upon the iron plate like quickſilver, for the iron communicates heat ſo very faſt to the water, that the adjoining parts are immediately converted into vapour, which prevents the water from coming into contact with the iron. Hence alſo it happens, than when a large lump of red hot iron is thrown into water contained in a pewter or wooden veſſel, it will melt, or burn a hole in the bottom, becauſe the conſtant atmoſphere of vapour around it keeps off the water all the while, and the iron is but ſlowly robbed of its heat.

But when we heat a large quantity of a fluid in a veſſel, in the ordinary manner, by ſetting it on the fire, we have an opportunity of obſerving ſome other phenomena which are very inſtructive. The fluid is gradually heated, and at leaſt attains that temperature which it cannot paſs without putting on the form of vapour. In theſe circumſtances, we always obſerve that it is thrown into a violent agitation, which we call boiling. This agitation continues as long as we throw in more heat, or any of the fluid remains, and its violence is proportional to the celerity with which the heat is ſupplied.

Another peculiarity attends this boiling of fluids, which, when firſt obſerved, was thought very ſurpriſing. However long and violently we boil a fluid, we cannot make it in the leaſt hotter than when it began to boil. The thermometer always points at the ſame degree, namely, the vaporific point of that fluid. Hence the vaporific point of fluids is often called their boiling point.

When theſe facts and appearances were firſt obſerved, they ſeemed ſurpriſing, and different opinions were formed with reſpect to the cauſes on which they depend. Some thought that this agitation was occaſioned by that part of the heat, which was more than the water was capable of receiving, and which forced its way through, ſo as to occaſion the agitation of boiling; others again imagined, that the agi-

tation proceeded from air, which water is known to contain, and which is now expelled by the heat.

Neither of theſe accounts however, is juſt or ſatisfactory; the firſt is repugnant to all our experience in regard to heat: We have never obſerved it in the form of an expanſive fluid like air. It pervades all bodies, and cannot be confined by any veſſel, or any ſort of matter, whereas the elaſtic matter of boiling water can be confined by external preſſure, as is evident in the experiments made with Papin's digeſter.

Yet, notwithſtanding the incompatibility of this explanation with our moſt familiar experience, it had a very general currency. No wonder that ſo ill-founded an opinion was accompanied by conſequences and deductions equally fanciful. Even philoſophers gave credit to the vulgar notion, that the veſſel in which water boiled with violence has very little heat in it, becauſe, ſay they, the fire runs up through it as through a ſieve, without heating it; therefore, when the finger is applied to the bottom of a boiling tea-kettle, it is felt no more than moderately warm. But this is a ſilly miſtake. The trial is commonly made with a ſort of fearful hurry, the fire all the while affecting the back of the hand, and obliging us to withdraw it before we can form any diſtinct opinion of the matter. But the great cauſe of this vulgar opinion is, that the bottom of a tea-kettle is uſually covered with a ſpongy cruſt of ſoot, which is almoſt the worſt conductor poſſible of heat. If it be ſcraped clean, and eſpecially if the wet finger be applied to it in that ſtate, it will be ſcalded in an inſtant.

The ſecond explanation is equally unſatisfactory, nor is it intelligible; for, if we continue to apply heat to the water, it will continue to boil to the laſt drop, and we ſhould therefore conclude that it is all air. But to be ſatisfied that this opinion is not true, we need only apply cold, or a cooling cauſe, to this ſuppoſed air, and we ſhall immediately find it condenſible into the ſame quantity of water from which it was produced.

A more juſt explanation will occur to any perſon, who will take the trouble to conſider this ſubject with patience and attention. In the ordinary manner of heating water, the heating cauſe is applied to the lower parts of the fluid. If the preſſure on the ſurface be not increaſed, the water ſoon acquires the greateſt heat which it can bear, without aſſuming the form of vapour. Subſequent additions of heat, therefore, in the ſame inſtant in which they enter the water, muſt convert into vapour that part which they thus affect. As theſe additions of heat all enter at the bottom of the fluid, there is a conſtant production of elaſtic vapour there, which, on account of its weighing almoſt nothing, muſt riſe through the ſurrounding water, and appear to be thrown up to the ſurface with violence, and from thence it is diffuſed through the air. The water is thus gradually waſted, as the boiling continues, but its temperature is never increaſed, at leaſt in that part which remains after long continued and violent boiling. The parts, indeed, in contact with the bottom of the veſſel may be ſuppoſed to have received a little more heat, but this is inſtantly communicated to the ſurrounding water through which the elaſtic vapour riſes.

This has the appearance of being a ſimple, plain, and complete account of the production of vapour, and of the boiling of fluids; and it is the only account that was given of this ſubject before I began to deliver theſe lectures: But I am perſuaded that it is by no means a full account of the matter. According to this account, and the notion that was conceived of the formation of vapour, it was taken for granted that, after a body is heated up to its vaporific point, nothing further is neceſſary but the addition of a little more heat to change it into vapour. It was alſo ſuppoſed, on the other hand, that when the vapour of water is ſo far cooled as to be ready for condenſation, this condenſation, or return into the ſtate of water, will happen at once, or in conſequence of its loſing only a very ſmall quantity of heat.

But I can eaſily ſhew, in the ſame manner as in the caſe of fluid-

ity, that a very great quantity of heat is neceſſary to the production of vapour, although the body be already heated to that temperature which it cannot paſs, by the ſmalleſt poſſible degree, without being ſo converted. The undeniable conſequence of this ſhould be, an exploſion of the whole water, with a violence equal to that of gunpowder. But I can ſhew, that this great quantity of heat enters into the vapour gradually, while it is forming, without making it perceptibly hotter to the thermometer. The vapour, if examined with a thermometer, is found to be exactly of the ſame temperature as the boiling water from which it aroſe. The water muſt be raiſed to a certain temperature, becauſe, at that temperature only, is it diſpoſed to abſorb heat; and it is not inſtantly exploded, becauſe, in that inſtant, there cannot be had a ſufficient ſupply of heat through the whole maſs. On the other hand, I can ſhew that when the vapour of water is condenſed into a liquid, the very ſame great quantity of heat comes out of it into the colder matter by which it is condenſed; and the matter of the vapour, or the water into which it is changed, does not become ſenſibly colder by the loſs of this great quantity of heat. It does not become colder in proportion to the quantity of heat obtainable from it during its condenſation.

All this will become evident, when we conſider with attention the gradual formation of vapour, in conſequence of the continued application of a heating cauſe, and the like gradual condenſation of this vapour, when we continue to apply to it a body that is colder. Thus, if we apply a heating cauſe to a quantity of water,—ſuppoſe by ſetting a tea-kettle with water upon the fire, the heat flows into it very faſt, from the beginning of the experiment until the water is heated to its boiling point. Perhaps, during the laſt five minutes, we find that the water has increaſed its temperature 20 degrees. It is a matter of general obſervation, that the paſſage of heat from one body to another is nearly in proportion to their difference of temperature, other circumſtances remaining the ſame. In the preſent caſe, therefore,

ſince the water does not ſenſibly change its temperature during the boiling, we may conclude that the heat continues to flow into it nearly at the ſame rate, and that it receives four degrees of heat in every minute. This ſuppoſition leads to no ſenſible error; for I have repeatedly found that, when water roſe 20 degrees during the laſt five minutes, it took 40 to gain 162 degrees of heat; and when I obſerved it to riſe 38 degrees in the ſame time, it had riſen 162 degrees in 20 minutes *. Now, if the common opinion were juſt, it is plain, that, in a few minutes more, the whole water would aſſume the form of vapour, and would produce a moſt violent exploſion, even ſufficient to blow up the houſe. Nothing like this happens; for, after ſeveral minutes have elapſed, we ſhall find that only a very ſmall quantity of water has become vapour, and that a great while indeed muſt elapſe, and a continual application of the fire muſt be made, before the whole of it can be made to aſſume that form. In the ordinary courſe of ſuch experiments in this climate, it requires about ſix times the number of minutes to boil off a ſmall quantity of water, that it takes to bring it to boil. I can ſcarcely remember the time in which I had not ſome confuſed idea of this diſagreement of the fact with the common opinion; and I preſume that it has come acroſs the mind of almoſt every perſon who has attended to the boiling of a pot or pan. But the importance of the ſurmiſe never ſtruck me with due force, till after I had made my experiments on the melting of ice. The regular procedure in that caſe, and its ſimilarity to what appears here, encouraged me to expect a ſimilar regularity in the boiling of water, if my conjecture was well founded. But almoſt every time I thought of it, it appeared to me ſo difficult, if at all poſſible, to procure a ſupply of heat that ſhould be tolerably uniform, or to aſcertain its irregularities, that I was diſcouraged from making an experiment which

* In both of theſe caſes, the heat had entered ſomewhat faſter in the beginning of the experiment, while the temperature of the water was lower.

EDITOR.

would in all probability be ſo anomalous. But I was one day told by a practical diſtiller, that, when his furnace was in good order, he could tell to a pint, the quantity of liquor that he would get in an hour. I immediately ſet about boiling off ſmall quantities of water, and I found that it was accompliſhed in times very nearly proportional to the quantities, even although the fire was ſenſibly irregular.

I, therefore, ſet ſeriouſly about making experiments, conformable to the ſuſpicion that I entertained concerning the boiling of fluids. My conjecture, when put into form, was to this purpoſe. I imagined that, during the boiling, heat is abſorbed by the water, and enters into the compoſition of the vapour produced from it, in the ſame manner as it is abſorbed by ice in melting, and enters into the compoſition of the produced water. And, as the oſtenſible effect of the heat, in this laſt caſe, conſiſts, not in warming the ſurrounding bodies, but in rendering the ice fluid; ſo, in the caſe of boiling, the heat abſorbed does not warm ſurrounding bodies, but converts the water into vapour. In both caſes, conſidered as the cauſe of warmth, we do not perceive its preſence: it is concealed, or latent, and I gave it the name of LATENT HEAT.

I ſhall now deſcribe a few of the experiments, which, I apprehend, will fully eſtabliſh the juſtice of my conjecture.

Experiment 1ſt. I procured ſome cylindrical tin-plate veſſels, about 4 or 5 inches diameter, and flat-bottomed. Putting a ſmall quantity of water into them, of the temperature 50 degrees, I ſet them upon a red hot kitchen table, that is, a caſt-iron plate, having a furnace of burning fuel below it, taking care that the fire ſhould be pretty regular. After four minutes, the water began ſenſibly to boil, and in twenty minutes more, it was all boiled off. This experiment was made 4th October 1762.

Experiment 2d. Two flat-bottomed veſſels, like the former, were ſet on the iron plate, with eight ounces of water in each, of the tem-

perature 50°. They both began to boil at the end of three and a half minutes, and in eighteen minutes more, all the water was boiled off.

Experiment 3d. The ſame veſſels were again ſupplied with twelve ounces of water in each, alſo of the temperature 50°. Both began to boil at the end of ſix and a quarter minutes, and the water was all boiled off, from the one in twenty-eight minutes, and from the other in ſomething more than twenty-nine.

I reaſoned from theſe experiments in the following manner: The veſſels in the firſt experiment received 162 degrees of heat in four minutes, or 40½ degrees each minute. If we, therefore, ſuppoſe that the heat enters equally faſt during the whole ebullition, we muſt ſuppoſe that 810 degrees of heat have been abſorbed by the water, and are contained in its vapour. Since this vapour is no hotter than boiling water, the heat is contained in it in a latent ſtate, if we conſider it only as the cauſe of warmth. Its preſence is ſufficiently indicated, however, by the vaporous or expanſive form which the water has now acquired.

In experiment ſecond, the heat abſorbed, and rendered latent, ſeems to be about 830.

In the third experiment, the heat abſorbed ſeems to be ſomewhat leſs, viz. about 750. The time of riſing to the boiling heat, in experiment third, has nearly the ſame proportion to that in experiment firſt, that the quantities of water have. The deficiency, therefore, in the heat abſorbed, has been probably only apparent, and ariſing from irregularity in the fire. Upon the whole, the conformity of their reſults with my conjecture was ſufficient to confirm me in my opinion of its juſtice. In the courſe of further experiments made both by myſelf and by ſome friends, and in which the utmoſt care was taken to procure a perfect uniformity in the heat applied, the abſorption was found extremely regular, and amounted at an average to about 810 degrees.

There are other caſes where this abſorption appears in a much

more ſingular manner. I put into a very ſtrong phial, about as much water as half filled it, and I corked it cloſe. The phial was placed in a ſand-pot, which was gradually heated, until the ſand and phial were ſeveral degrees above the common vaporific point of water. I was curious to know what would be the effect of ſuddenly removing the preſſure of the air, which is well known to prevent water from boiling. The water boiled a very ſhort while, but the ebullition gradually decreaſed, till it was almoſt inſenſible. Here the formation of more vapour was oppoſed by a very ſtrong preſſure proceeding from the quantity of vapour already accumulated, and confined in the upper part of the phial, and from the increaſed elaſticity of this vapour, by the increaſe of its heat. When matters were in this ſtate, I drew out the cork. Now, according to the common opinion of the formation of vapour by heat, it was to be expected that the whole of the water would ſuddenly aſſume the vaporous form, becauſe it was all heated above the vaporific point. But I was beginning by this time to expect a different event, becauſe I could not ſee whence the heat was to be ſupplied, which the water muſt contain when in the form of vapour. Accordingly, it happened as I expected; a portion only of the water was converted into vapour, which ruſhed out of the phial with a conſiderable exploſion, carrying along with it ſome drops of water. But, what was moſt intereſting to me in this experiment was, that the heat of what remained was reduced in an inſtant to the ordinary boiling point. Here, therefore, it was evident that all that exceſs of heat which the water had contained above the boiling point, was ſpent in converting only a portion of it into vapour. This is plainly inconſiſtent with the common opinion, that nothing more is neceſſary for water's exiſting in a vaporous form under the preſſure of the atmoſphere, than its being raiſed to a certain temperature. The experiment makes it more probable, that if the influx of heat could at that inſtant have been prevented, it would have remained in the form of water, although raiſed, in a very ſenſible degree, above the boiling temperature.

I was anxious to learn whether the heat which diſappeared in this experiment was in an accurate proportion to the quantity of vapour produced, or the quantity of water that had diſappeared. But the drops of water that were hurried along by the exploſion, without being converted into vapour, made it impoſſible for me to aſcertain this with any tolerable accuracy, although I repeated the experiment ſeveral times.

This experiment was afterwards made by my friend Mr. Watt, in a very ſatisfactory manner. His ſtudies for the improvement of his ſteam-engine, gave him a great intereſt in every thing relating to the production of ſteam. He put three inches of water into a ſmall copper digeſter, and, ſcrewing on the lid, he left the ſafety-valve open. He then ſet it on a clear fire of coaks, and after it began to boil and produce ſteam, he allowed it to remain on the fire half an hour, with the valve open. Then, taking it off the fire, he found that an inch of water had boiled away. In the next place, he reſtored that inch of water, ſcrewed on the lid, and ſet it on the fire; and as ſoon as it began to boil, he ſhut the ſafety-valve, and allowed it to remain on the fire half an hour as before. The temperature of the whole was many degrees above the boiling point. He took it off the fire, and ſet it upon aſhes, and opened the valve a very ſmall matter. The ſteam ruſhed out with great violence, making a ſhrieking noiſe for about two minutes. When this had ceaſed, he ſhut the valve, and allowed all to cool. When he opened it, he found that an inch of water was conſumed.

It is reaſonable to conclude from this experiment, that nearly as much heat was expended during the blowing of the ſteam-pipe, as had been formerly expended in boiling off the inch of water. For, before opening the valve, the temperature was many degrees above the boiling point, and all this diſappeared with the vapour. The ſame inference may be drawn from the time that the digeſter continued upon the fire with the valve ſhut, becauſe we may conclude that the heat

was entering nearly at the ſame rate during the whole time. It is plain, however, that the experiment is not of ſuch a kind as to admit of nice calculation; but it is abundantly ſufficient to ſhew that a prodigious quantity of heat had eſcaped along with the particles of vapour produced from an inch of water. The water that remained could not be hotter than the boiling point, nor could the veſſel be hotter, otherwiſe it would have heated the water, and converted it into vapour. The heat, therefore, did not eſcape along with the vapour, but *in* it, probably united to every particle, as one of the ingredients of its vaporous conſtitution. And as ice, united with a certain quantity of heat, is water, ſo water, united with another quantity of heat, is ſteam or vapour.

There are ſome other inſtances of the ſame kind, which may perhaps appear ſtill more ſtriking, I mean the experiment made upon the boiling of fluids *in vacuo*, which are eaſily explained upon theſe principles, and ſerve at the ſame time to ſupport them. The firſt perſon who made experiments of this kind, was the Honourable Mr. Boyle. He placed phials with hot water, which had been well boiled to expel its air, under the receiver of his new invented air-pump, and then exhauſted the air. Long before the vacuum was nearly perfect, the water began to boil, and continued to do ſo with great violence while he worked the pump. He expreſſes his ſurpriſe at the great heat which the receiver acquired, and at the ſudden diminution of the heat of the water, which he expected to find ſtill ſcalding hot when he admitted the air and took off the receiver, whereas it was only lukewarm. This experiment has been repeated by others many hundreds of times, and with more particular attention to the different circumſtances. They all agree in this, that the water, from being hot, becomes lukewarm immediately, or very faſt, and plainly in conſequence of its boiling or producing vapour.

At the time that I was engaged in theſe experiments, Profeſſor Robiſon, then one of my pupils, told me, that in ſome experiments which

he had made with the air-pump belonging to the univerſity of Glaſgow, however hot the water was at firſt, the heat of it was rapidly diminiſhed, till it fell to the 90th or 91ſt degree of Fahrenheit. Mr. Watt alſo aſſured me, that, in a very good vacuum, the water will continue to boil or produce copious vapour, till its heat be quickly reduced to the 70th degree of Fahrenheit. This ſudden diſappearance, therefore, of a great part of the heat of the water, while a very ſmall part only aſſumes the vaporous form, is an additional proof of what I have advanced concerning the manner in which heat produces vapour.

Similar to theſe examples, are alſo, in my opinion, ſome ſurpriſing experiments made by Dr. Cullen, with ſome very volatile fluids, and publiſhed by him in the Phyſical and Literary Eſſays of Edinburgh. The Doctor employed the late Dr. Dobſon, at that time his pupil, to make experiments on the heat or cold produced by mixing different fluids and ſolids with one another. Dr. Dobſon, in making theſe experiments, obſerved that the thermometer, when lifted out of many of the fluids, and ſuſpended a ſhort time in the air beſide them, fell down to a lower degree than that indicated by another thermometer which hung conſtantly in the ſame air. And, after varying his obſervations on this phenomenon, he found reaſon to conclude, that it was occaſioned by the evaporation of the laſt drop of fluid which adhered to the bulb of the thermometer; the ſinking of the thermometer being always greateſt, and moſt quickly produced, when this inſtrument was taken out of the moſt volatile fluids. Dr. Cullen had then a curioſity to try whether the ſame phenomenon would appear on repeating theſe experiments under the exhauſted receiver of the air-pump; and to ſatisfy himſelf, he placed on the plate of the air-pump, a glaſs goblet containing water, and in the water he placed a wide mouthed phial, containing vitriolic æther. The whole was covered with an air-pump receiver, having, at the upper end, a collar of leathers, in a braſs ſocket, through which a thick ſmooth wire could

be moved, and from the lower end of this wire, projecting into the receiver, was ſuſpended a thermometer. By puſhing down the wire, the thermometer could be dipped into the æther; by drawing it up, it could be taken out and ſuſpended over the phial.

The apparatus being thus adjuſted, the air-pump was worked to extract the air. An unexpected phenomenon immediately appeared, which prevented the experiment being made in the manner intended. The æther was thrown into a violent agitation, which Dr. Cullen imputed to the extrication of a great quantity of air. It is evident, however, from ſeveral particulars, that the æther was not emitting air principally, but a great quantity of æthereal vapour, and that it was truly in a boiling ſtate: For the elaſtic fluid, extricated by working the pump, had a very ſtrong odour of æther, and the quantity of the æther in the phial, was conſiderably and ſuddenly diminiſhed, while this boiling continued. And, what is eſpecially to be attended to at preſent, the æther became ſuddenly ſo cold, that it froze the water in the goblet around it, although the temperature of the air, and all the materials, were at the 54th degree of Fahrenheit, in the beginning of the experiment.

You will now eaſily underſtand how the phenomenon may be explained, by the principles which I have already eſtabliſhed. It is evident, that the vitriolic æther is a fluid of ſuch great volatility, that, were it not compreſſed by the weight of the atmoſphere, which forces the parts of it to remain together, it would for ever be in the form of vapour, in all the variations of heat or cold, which we commonly experience; that the proper vaporific heat of this fluid, or the loweſt degree at which it would boil, were it totally free from the preſſure of the atmoſphere, is far below the freezing point of water. It is therefore always more than hot enough to boil and produce vapour, whenever we take off the preſſure of the atmoſphere from its ſurface. Accordingly, when we take off this preſſure from it, vapour begins immediately to be produced, and ſuch a great quantity of the

ſenſible heat that was in the æther, is ſuddenly changed into latent heat, or abſorbed in the vapour, that the remaining æther muſt neceſſarily become exceſſively cold. It may become ſo cold at laſt, as to be no longer in a condition to produce more vapour even *in vacuo*.

This experiment, therefore, has a good deal of analogy with the production of cold, by mixing ſnow with different ſalts. In thoſe experiments, the ſnow is made ſuddenly to aſſume the form of a fluid, without adding heat to it. In the above experiment, the æther is made ſuddenly to aſſume the form of vapour, without its receiving any addition of heat.

We may further remark, that the above ſuppoſition, with regard to the lowneſs of the proper vaporific point of the vitriolic æther, is not aſſumed without good reaſon: it is ſupported by the analogy of the proper vaporific heats of other fluids. All of theſe which have hitherto been tried begin in a vacuum to boil, and emit vapour, when their heat is lower, by 120 degrees at leaſt, than their vaporific point under the preſſure of the atmoſphere. But the vitriolic æther is well known to boil under the preſſure of the atmoſphere at about 100 degrees of heat. We have reaſon therefore to conclude that it will boil *in vacuo* ſo long as its heat is higher than the 20th degree below the beginning of Fahrenheit's ſcale, or the 52d degree below the cold ſufficient for the freezing of water.

The more carefully we conſider the production of expanſive vapour, in all that variety of circumſtances that have been already narrated, and many others of the ſame nature, the more ſatisfactory evidence have we, that a great quantity of ſenſible heat is ſomehow abſorbed by the water, in paſſing from the liquid to the vaporous form, or diſappears in ſome more unaccountable manner during its formation. For it does not immediately appear in the ſteam. This is evident by placing the bulb of a thermometer in the way of the ſteam, ſo as to be completely ſurrounded by it. It raiſes the liquor in the ſtem of the thermometer no higher than it would be raiſed by plunging it in

the boiling water. As a farther proof that it is no hotter, it is a matter of daily experience, that the ſteam of boiling water does not ſcorch the moſt delicate flowers, nor ſpoil, in the ſmalleſt degree, the fragrance or delicacy of their odorous principle. On the contrary, we obtain this principle in its greateſt perfection, by placing thoſe flowers in the upper part of a common ſtill, in which water is made to boil. The heat of the paſſing ſteam volatilizes, and carries off along with it the precious aroma; and it is found in the water condenſed by the worm plunged in a refrigeratory filled with cold water. This proceſs is found to be vaſtly preferable (for ſuch plants as are uncommonly delicate) to the immerſing them in the boiling water. It is plain, therefore, that the great quantity of the heating principle, which we ſuppoſe to be flowing continually from the fuel in the furnace into the water contained in the ſtill, is not to be found in the ſteam, in the ſame active form, or as the cauſe of heat. But it is not loſt, and is really contained in the ſteam, in ſuch a manner as to manifeſt itſelf by its former effects, by heating any body when the vapour is again converted into water. As a particle of water, in the inſtant of its becoming a particle of vapour, attracts and unites with itſelf one or more atoms of this cauſe of heat, if only raiſed to the requiſite temperature, and retains them as parts or ingredients of its vaporous form, ſo we may reaſonably conjecture, that when a particle of vapour again becomes water, theſe atoms of heat are ſet at liberty by the fixed laws of chemical affinity, and are then at liberty to enter into any other body which is in a fit condition for receiving them. Such a body ſhould be warmed by them, and this ſhould be ſenſible by means of the thermometer. If indeed the body be on the verge of liquefaction or ebullition, it may be as much diſpoſed to abſorption of heat as the actually collapſing vapour of water is diſpoſed to its emiſſion. Yet, in this caſe alſo, we ſhould obtain equally incontrovertible indications of the extrication of heat from the elaſtic ſteam. The temperature of the body may not be raiſed, but it will melt or boil.

When this opinion got hold of my mind, I was peculiarly pleaſed with the explanation which it gave me of many curious facts obſerved in diſtillations, and other chemical proceſſes, which greatly perplexed the chemiſts, who being ignorant of their cauſe, could neither explain them, nor avoid them when hurtful. The multitude of important conſequences which ſeemed to reſult from this combination of heat with the volatilized body, made me anxious to obtain accurate information; and the road that I was to follow ſeemed to be very obvious and eaſy. The object was to obtain, not only certain proof of the extrication of a great quantity of heat contained in the vapour, and not perceivable in it by means of the thermometer, but alſo to diſcover whether all that heat was to be obtained from vapour while condenſing, which had diſappeared during its formation. The propoſed analyſis was perfectly ſimilar to what I had accompliſhed, ſo much to my ſatisfaction, in the congelation of liquids compared with the liquefaction of the frozen maſs: and the mode of proceeding was equally obvious and familiar: I had only to attend to what happens in the working of a common ſtill.

All of you know well enough how the operation of common diſtilling is conducted. The water, or other liquor to be diſtilled, is put into a boiler, which is ſet on the fire. The vapour produced by its ebullition is not allowed to riſe and mix with the air, but is confined by the head of the boiler, called the ſtill, and it is conducted laterally by a pipe. But if nothing more were done, the vapour would only blow out through this pipe, as we obſerve it to do from the ſpout of a tea-kettle, when it boils with violence. This pipe is, therefore, inſerted into another, which is made to paſs through a large veſſel, called the Refrigeratory, or Cooler, but without communicating with it. The refrigeratory is filled with cold water. This, being in contact with every part of the pipe, keeps it cold, and the pipe being made of metal, which is known to tranſmit heat very faſt, the inſide of it is quickly robbed of any heat which the vapour can communicate

to it; and this heat is tranſmitted to the water in the refrigeratory, and muſt raiſe its temperature. That this may be more copiouſly done, the pipe is generally made of a ſpiral form, like a cork-ſcrew, its upper end communicating with the pipe from the ſtill-head, and its lower end being turned ſidewiſe, and led through the ſide of the refrigeratory, and a veſſel is ſet below its mouth, to receive the liquor formed of the condenſed vapour.

From this brief deſcription of the apparatus (of which more particular notice will be taken in its proper place) you will eaſily comprehend the whole of the proceſs, and the circumſtance to which our particular attention muſt be directed. The heat of the fuel penetrates the body of the ſtill, raiſes the temperature of the water (which I ſhall ſuppoſe to be the ſubſtance under examination) till it can no longer remain in a liquid form, but boils, that is, it is converted into elaſtic vapour; and, if my opinion be juſt, it abſorbs, in the inſtant of converſion, or is converted by abſorbing, a prodigious quantity of heat, namely, as much as would raiſe its temperature about 800 degrees, if it acted on it in the ſame manner as before its boiling. I imagine that this heat is contained in the produced vapour, and goes off with it through the pipe. During its paſſage along the convolutions of the ſpiral pipe, or worm, as it is called, the hot vapour comes in contact with the circumference of the cold pipe. The fact is, that it is completely condenſed by this contact. For, when things are properly conducted, by giving a very extenſive ſurface of contact, and keeping a plentiful ſupply of cold water in the refrigeratory, nothing iſſues from the mouth of the pipe but cold water, and the air which occupied the upper part of the apparatus, in the beginning of the diſtillation, and a ſmall quantity which is contained in almoſt all waters, and is extricated from them by boiling. In the mean time, it is a fact, that the water in the refrigeratory grows warmer and warmer as the diſtillation goes on, and indeed ſoon grows extremely hot, if it be not allowed to run off, and its place be not ſupplied by a

continued ſtream of cold water. When this is not carefully ſecured, the liquor which drops from the worm is warmer and warmer, in the ſame proportion as the water in the refrigeratory (for it evidently cannot be colder, but muſt rather be ſomewhat warmer), and at laſt it is almoſt boiling hot. Now the condenſation ceaſes, and ſteam blows through the worm in abundance. This has ſometimes happened in the diſtillation of ſpiritous liquors, and has occaſioned fatal accidents, becauſe theſe vapours are inflammable.

Such are the facts. My notion of the internal procedure was this: The particles of vapour which come into immediate contact with the circumference of the ſpiral pipe, having a ſmaller attraction for the heat combined with them, than thoſe of the cold pipe have, allow them to unite with the ſubſtance of the pipe, whence they are ſoon tranſmitted to the ſurrounding water, ſo that the pipe is ſtill in a condition to rob the ſubſequent particles of vapour of their natural heat alſo. The particles of vapour, deprived of their latent heat, are no longer vapour, but water, which falls to the lower ſide of the worm, and trickles down along it. The exterior particles of vapour being thus removed out of the way, thoſe which were nearer to the middle, or axis of the pipe, being puſhed forward by the ſteam behind them, and, at the ſame time expanding, come, in their turn, to be applied to the ſides of the pipe, and are alſo condenſed into water; and thus, the different ſtrata of vaporous particles, from the circumference to the axis, come, in turn, to be deprived of their heat, and, by the deprivation, become water again, cooled down to the temperature of that part of the pipe by which they are condenſed. Theſe ſtrata of vapour may be conceived as like the ſliding tubes of a pocket ſpy-glaſs; thoſe next the axis always getting beyond the ſtratum which ſurrounded them, before they expand and are condenſed by touching the ſides. By this proceſs, the upper parts of the worm, and the ſurrounding water, become ſooneſt hot, and are always hotter than thoſe farther down the refrigeratory (if the water be not ſtirred). It is alſo reaſonable to

ſuppoſe that thoſe parts of the pipe which are coldeſt condenſe the paſſing vapours moſt copiouſly, and that, when heated very near to the temperature of boiling water, they condenſe it very ſlowly indeed. Therefore more uncondenſed vapour paſſes forward, to a part not yet ſo warm, where it is condenſed, and by giving out its heat, that part of the pipe is more quickly heated; and thus the heating of the pipe and water proceeds to the bottom of the refrigeratory.

If this conception of the proceſs be juſt, it ſhould follow that no heat ſhould diſappear in the experiment, except what is unavoidably loſt by warming the veſſels, and, by their intervention, warming the ſurrounding air, and being carried off by it. All the heat tranſmitted from the fuel ſhould be found in the diſtilled water, and in the water of the refrigeratory, except the loſs by waſte now mentioned.

Thus were the ſteps of my examination plainly pointed out to me. I muſt firſt meaſure the heat expended in boiling off a certain quantity of water. I muſt, in the ſecond place, meaſure the heat communicated to the diſtilled water, and to the water of the refrigeratory; and I muſt compare thoſe quantities together, and find them equal, or unequal.

When, for example, we ſet a ſtill to work, the refrigeratory of which contains 100 gallons of water, of the temperature 50°, and we diſtil off one gallon; if we now examine the water of the refrigeratory, and find that it has gained 11 degrees of heat, we muſt reaſon in this way: Had the ſteam, while it heated this water, produced no more effect than in proportion to its perceptible heat, and the quantity of water which compoſed it, which is one gallon, it ſhould not have increaſed the heat of the boiling water more than $1\frac{1}{2}$ degree. For it is little more than 150 degrees perceptibly hotter (the diſtilled water having the temperature 61° nearly). But it has actually raiſed the temperature of 100 gallons 11 degrees. Here, therefore, are $9\frac{1}{2}$ degrees of heat added to each of 100 gallons of cold water, without our being able to account for it according to what has been the com-

mon opinion. But a quantity of heat which is ſufficient for increaſing the ſenſible heat of 100 gallons of water by 9½ degrees, could it be all put into one gallon, without converting it into vapour, would be ſufficient for increaſing the ſenſible heat of that gallon, by 950 degrees. And all this heat was actually contained in the one gallon, whilſt in the form of vapour, and without ſhewing itſelf by affecting the thermometer more than boiling water does.

I made ſeveral experiments, in conformity to thoſe already mentioned, on the converſion of water into ſteam, which fully ſatisfied my own mind that my opinion concerning the nature of elaſtic vapour was juſt. Indeed, when my mind was occupied by this thought, conviction flaſhed on me from every quarter, that the quantity of heat in all vapours was prodigiouſly greater than what was indicated merely by their ſenſible heats or temperatures. Every perſon knows the ſcalding power of ſteam, and that a momentary puff of it from the ſpout of a tea-kettle, which will ſcarcely render the hand damp, and does not contain the fourth part of one drop of water, will in an inſtant raiſe the whole hand into a bliſter, which a thouſand drops of boiling water could not do. There is hardly a perſon who has not been ſtruck by the great heat produced in the refrigeratory of a common ſtill; and thoſe who manufacture ſpiritous liquors for the market have often found as great difficulty and expence attending the ſupply of cold water for their refrigeratory, as they had in ſupplying their furnace with fuel. This has prevented the erection of ſuch manufactures in great cities, where it would otherwiſe be moſt convenient and profitable; becauſe they cannot be ſupplied with cold water. The more I reflected on theſe things, I was the more aſtoniſhed that a thing ſo obvious, and ſo intereſting to a number of active people, attentive to their intereſt, and accuſtomed to eſtimate every thing by meaſurement of ſome kind, had never attracted their attention, and engaged them, or ſpeculative men, who are always ready to aſſiſt them, in ſome compariſon between the fuel expended and the re-

markable heats produced. It now appeared wonderful to me, that a competent knowledge of this ſubject was not acquired almoſt as ſoon as the ſcale of the thermometer was determined by the fixed points of boiling and freezing water. And, when that noble machine the ſteam engine, was invented, and was continually in the hands of moſt ingenious men, ſuch as Dr. Hooke, Dr. Papin, Dr. Deſaguilliers, and others, and almoſt preſcribed this examination as indiſpenſably neceſſary for its very practicability, it is ſtill more ſurpriſing, that ſo obvious a thing never occurred. But no ſuch examination is to be met with in the writings of either the mechanicians or the chemiſts.

The latent heat of ſteam ſeemed to me to be ſo completely eſtabliſhed, that I was little ſolicitous of more experiments for my own conviction, or for making the doctrine clearly comprehenſible to others. I, therefore, taught it in my annual courſes, and have reaſon to believe that it was perfectly underſtood by my ſtudents. But it was a deſirable thing to obtain exact meaſures of the heat contained in ſteam, becauſe this appeared to me the moſt accurate way for obtaining meaſures of the heat produced by fuel, and might be of great ſervice in making trials of the moſt economical methods of applying fuel, a point of immenſe conſequence in nine-tenths of all our manufactures. It was ſome conſiderable time, however, before I could execute this to my ſatisfaction, owing to the occupations of a double academical duty, and the attention due to my patients. And, at laſt, indiſpoſition obliged me to commit the conduct of the experiments to one of my pupils, the late Dr. Irvin of Glaſgow, a young gentleman of an inquiſitive and philoſophical mind, of great ingenuity, and peculiarly qualified for this taſk, by the habits of mathematical ſtudy, and ſcrupulous attention to all kinds of meaſurement. The following was the firſt of the experiments for this purpoſe, which I made in Glaſgow, with the aſſiſtance of Mr. Irvin, on the 9th of October 1764.

Five meaſures (each containing 4 lb. 5 oz. and 6 dr. avoirdupois)

of water, of the temperature 52°, were poured into a ſmall ſtill in the laboratory. The fire had been kindled about 40 minutes before, and was come to a clear and uniform ſtate. The ſtill was ſet into the furnace, and, in an hour and twenty minutes, the firſt drop came from the worm; and in three hours and forty-five minutes more, three meaſures of water were diſtilled, and the experiment ended. The refrigeratory contained 38 meaſures of water, of which the temperature, at the beginning of the experiment, was 52°. When one meaſure had come over, the water in the refrigeratory was at 76°. When two had come over, it was at 100°; and when three had come over, it was at 123°.

In this experiment, the heat, which emerged from three meaſures of water, had raiſed the temperature of the water in the refrigeratory from 52° to 123°, or 71°. Now 3 is to 38 as 71 to 899⅓, and the heat would have raiſed the three meaſures 899⅓ degrees in its temperature, if this had been poſſible without converting it into vapour. The heat of the vapour from which this emerged, was 212°, or 160° more than that of the water. Taking this from 899°, there remains 739°, the heat contained in the vapour in a latent ſtate.

But this muſt be ſenſibly leſs than the truth. During the experiment, the veſſels were very warm—the head of the ſtill as hot as boiling water, and the refrigeratory gradually riſing from 52°, which was within a degree or two of the temperature of the air of the laboratory, to 123°. A very conſiderable portion of the latent heat of the ſteam muſt have been carried off by the air in contact with a conſiderable ſurface, ſome of which was exceedingly hot. A great deal muſt alſo have been carried off in the ſteam which aroſe very ſenſibly from the water in the refrigeratory, towards the end of the experiment. Mr. Irvin alſo obſerved, that, during the diſtillation, the temperature of the water which ran from the worm, was about 11° hotter than the water in the refrigeratory. The ſteam, therefore, at a medium, was not 160° hotter than the water which ran from the worm, but 125°, its mean temperature being about 87°. This conſideration

alone will make the latent heat of the ſteam not leſs than 774 degrees, without any allowance for waſte.

Some compariſon may alſo be made between the heat expended in the production of the ſteam, and that which emerges during its condenſation. The time which elapſed during the raiſing of the temperature of the five meaſures of water from 52° to 212°, that is 160°, was one hour and twenty minutes, or 80′,—and 225′ elapſed during the boiling off of three meaſures. Therefore, ſince 80 is to 225 as 160 to 450, as much heat was expended as would have raiſed the five meaſures 450° in temperature. This would have raiſed three meaſures 750° above the boiling heat already produced. This gives 750 for the latent heat of the ſteam, beſides what was unavoidably loſt by communication to the ambient air, and what was expended in heating the veſſels.

This is an outline of the experiments which muſt be made for aſcertaining this fundamental point. A few weeks after, Mr. Watt made ſimilar experiments with a ſmaller ſtill, better fitted for a trial of this queſtion, and much more manageable. The medium reſult of theſe trials gave 825° for the heat contained in the ſteam. I have narrated the procedure, that you may ſee the circumſtances which muſt be attended to, in order to draw a concluſion from them. What I have related is ſufficient for eſtabliſhing the main principle, namely, that the heat which diſappears, in the converſion of water into vapour, is not loſt, but is retained by the vapour, and is indicated by its expanſive form, although it does not affect the thermometer. This heat emerges again from this vapour when it becomes water, and recovers its former quality of affecting the thermometer; in ſhort, it appears again as the cauſe of heat and expanſion. But much remains to be done, before the experiment can be made uſe of for giving us a preciſe meaſure, either of the heat imparted from the fuel, or of that which emerges from the ſteam. A train of experiments muſt be made for obtaining the preciſe rate at which the heat of the fuel

paſſes into the water during the riſing of its temperature, in order that from thence we may conclude the rate at which it continues to enter while the water is diſtilling. Another train of experiments is neceſſary for aſcertaining the whole loſs of heat by the veſſels, and by communication to the air. To relate theſe would conſume much time, and they will readily occur to any perſon much converſant with chemical proceſſes.

I think it ſufficient to inform you, that Mr. Watt, in the courſe of his ſtudies on the ſteam-engine, has made all the neceſſary experiments with the moſt ſcrupulous care, knowing that the improvement of that noble engine muſt depend entirely on an exact knowledge of the procedure of nature in the formation and condenſation of ſteam. Mr. Watt informs me, that he has obſerved as exact coincidence between the heat rendered latent in the vapour, and that which emerges from it, as can be deſired; and that the heat obtainable from ſteam, capable of ſuſtaining the ordinary preſſure of the atmoſphere, is not leſs than 900 degrees of Fahrenheit's ſcale, and that it does not exceed 950.

But this is not the only way in which it may be meaſured; there being other indications of the preſence of heat, that are equally ſuſceptible of meaſure. I have already obſerved, that if the body employed for condenſing the ſteam be in an abſorbing ſtate, its temperature will not be increaſed by what is thus abſorbed. But, as this abſorption and new combination of heat, is accompanied by a change in the conſtitution of the body, which is ſufficiently remarkable, viz. the body being either melted or volatilized; and, as the quantity of matter thus changed in its form, muſt be proportional to the heat combined with it, it follows, that the heat which emerges from a given quantity of vapour, of the temperature 212°, will melt a preciſe quantity of ice, or any other body; and therefore, the quantity of heat latent in ſteam, may be compared with that contained in the boiling hot water from which it is produced, namely, by comparing the quantities of ice melted by both. This, accordingly, was one of the

firſt methods which occurred to me for the meaſurement, and it came naturally into my thoughts, as I had, not long before this, been employed in the analyſis of liquefaction. I projected ſome experiments for this purpoſe, to be tried in the enſuing froſts. But my friend Mr. Watt, being now warmly engaged in the ſame inquiry, which had become ſo intereſting to him, I was ſoon ſupplied by him with meaſures abundantly accurate for my views. The celebrated philoſopher, Lavoiſier, took this other method of meaſurement, and invented a moſt ingenious apparatus for the purpoſe of meaſuring all productions of heat, by the quantity of ice melted by thoſe operations of nature in which it appears. The calculations deduced from ſuch of his experiments as have come to my knowledge, gave a ſomewhat higher value of the latent heat of ſteam, making it 1000 degrees, or perhaps a little more.

It is unneceſſary to add any thing more to the preceding facts, to prove that the interruption in the calorific proceſs, in the converſion of fluids into elaſtic vapour in the act of ebullition, is perfectly ſimilar to that in the proceſs of liquefaction; and I flatter myſelf that we may now take it as a point fully eſtabliſhed, that, *when a fluid body is raiſed to its boiling temperature, by the continual and copious application of heat, its particles ſuddenly attract to themſelves a great quantity of heat, and, by this combination, their mutual relation is ſo changed, that they no longer attract each other, gathering into drops and forming a liquid, but avoid each other, ſeparating to at leaſt ten times their former diſtance, (for a cubic inch of water forms much more than a thouſand cubic inches of vapour), and would ſeparate much farther, were they not compreſſed by the weight of the atmoſphere; and in ſhort, they now compoſe a fluid, elaſtic, and expanſive, like air.*

This new form of aggregation is the effect of a new combination of heat with the primary particles of water, and is a ſufficient indication of this union, in the ſame manner as fluidity was a ſufficient mark of a ſudden and copious combination of heat with the particles

of ice. We cannot, perhaps, form a very diſtinct notion of the nature of this combination, ſo as to underſtand its diſtinction from that which takes place in all lower temperatures, and is accompanied only by expanſion of the ice, without any change of form. In the ſame manner, we do not know the difference between the mode of union, which takes place between heat and water in all temperatures, from 32° to 212°, and that which obtains whenever we attempt to increaſe this laſt degree of heat by a more copious ſupply. We muſt content ourſelves with obſerving the fact, and remembering that the form of elaſtic vapour is the characteriſtic of a very peculiar combination of heat with the elements of other bodies.

We had ſome difficulty in conceiving how 130 or 140 degrees of heat could combine with the particles of ice, without increaſing its bulk in the ſame proportion as it had done in lower temperatures, and as it continues afterwards to do in water; but, in the preſent caſe, we have no ſuch difficulty, becauſe the bulk of the compound being much more increaſed, than in the proportion of the preceding expanſion *, there ſeems to be more room than before, where it may be lodged without redundancy. We are not ſurpriſed, therefore, at finding that the temperature is not increaſed; for, inſtead of thinking that heat ſhould flow out, and enter a thermometer already expanded 212 degrees, we ſhould rather expect, that when the bulk has increaſed 1800 times, while only 900 degrees of heat are thrown into it, there would rather be a diſpoſition in the heat to quit the thermometer, and enter into this empty ſpace. I do not pretend to ſay that heat is

* Mr. Watt has made a great number of experiments, for aſcertaining the bulk into which a cubic inch of water is expanded when it compoſes the ſteam, which, at the temperature 212°, ſuſtains the preſſure of the atmoſphere; ſome of theſe were direct, by evaporating a known quantity of water—others were more circuitous, deduced from the performance of his engines. The medium reſult gave about 1800 cubic inches. We may ſay, that a cubic inch of water forms a cubic foot of ſuch ſteam. EDITOR.

contained in vapour, or in water, as water is contained in the pores of a ſponge *, but only that there does not appear any thing unaccountable in the fact, that this great accumulation of heat does not affect the thermometer. We now ſee, in ſome degree, how it cannot. The thermometer cannot riſe without being hotter; but, if hotter, it cannot condenſe ſteam; and if the ſteam be not condenſed, its heat cannot emerge, becauſe whatever cauſed the abſorption of heat maintains it abſorbed. But, whatever be the nature of the equilibrium of temperature among bodies, it remains unaffected by the change which the water has undergone in its form. Whenever a body hotter than 212° comes in contact with the vapour, it raiſes its temperature and expands it; and whenever a body colder than 212° touches it, the vaporific union is immediately diſſolved; heat quits the vapour, and it becomes water. The heat emerges in all its intenſity and abundance, if there be enough of colder matter to receive it, but not elſe. Therefore, although the ſteam from the mouth of a tea-kettle will not raiſe a thermometer above 212°, a ſingle puff of it, which does not contain one grain of water, will ſcald a conſiderable extent of the ſkin, as much as if it were plunged for a moment in boiling water. The rapidity with which this tranſition of a great quantity of heat is made, is wonderful, when the ſurface of cold matter is ſufficiently extenſive. Two Engliſh gallons of ice-cold water, daſhed in ſmall drops through the capacity of a ſteam cylinder holding three hogſheads, will condenſe the vapour which fills it, in leſs than one-fourth of a ſecond.

There are other ways, however, in which vapour may be condenſed, without drawing off its latent heat by means of cold bodies. It

* There are phenomena, indeed, which have induced many naturaliſts of reputation to think ſo. Air, ſteam, and other elaſtic fluids, raiſe the thermometer when ſuddenly and greatly compreſſed; but this inference is too haſty, and a proper examination of the union between theſe ſubſtances and heat, renders it extremely doubtful, whether this emiſſion of heat ſhould neceſſarily follow the compreſſion.

EDITOR.

may be condenſed by mechanical compreſſion, and in this caſe alſo we ſee its latent heat emerge.

This is a nice and troubleſome experiment, and requires a very nice apparatus. A metal cylinder, nicely bored, is fitted with a piſton; and the cylinder is made to terminate below, in a ſmall glaſs tube, cloſe at the end. A thermometer is ſo fixed to the cylinder, that its bulb is in the axis, and the tube projects outwards through the cylinder. A little water is poured into it, ſo as to fill the glaſs tube at the bottom. The piſton is put into the top, and a ſmall hole in the piſton is left open. A large glaſs cylinder is filled with water, kept boiling hot. The water in the glaſs tube is made to boil briſkly, the ſteam blowing through the hole in the piſton. In this ſtate it is immerſed in the boiling water, the hole in the piſton ſpeedily ſtopped, and the piſton thruſt down with great force, at leaſt half way. The thermometer riſes immediately, and the water in the glaſs tube is ſeen to increaſe in bulk. The whole muſt be done with great diſpatch.

We now ſee clearly why the boiling point of a fluid is ſo conſtant, and why no violence of fire can make water in an open veſſel hotter than 212 degrees. The inſtant that any portion of it attains that temperature, it abſorbs the heat as faſt as it can be tranſmitted through the veſſel, and becomes elaſtic vapour, which riſes through the reſt, and eſcapes. This rapid abſorption of heat is moſt diſtinctly perceived by means of the little philoſophical toy, called a pulſe-glaſs, the ingenious contrivance of Dr. Franklin. This conſiſts of two thin glaſs balls, A and B, (fig. 2.) connected by a ſlender pipe. One of the balls, and a ſmall part of the pipe, is filled with water; and this being made to boil violently, the ſteam blows all the air out of the other ball, and follows it. By ſticking the end of the pipe C into a bit of tallow, the hole is ſtopped up, and the ſteam in the ball B ſoon collapſes, and becomes water, ſo there is now a fine vacuum in this ball. The flame of a blow-pipe being now directed on the neck of the pipe

C, it is ſoon hermetically ſealed. Thus we have removed the preſſure of the atmoſphere from the ſurface of the water, and it will now boil by the heat of the hand, if the ball B be kept a little colder, that it may condenſe the ſteam as faſt as it comes into it. It will be a long time, however, before the warmth of the hand heats all the water in A ſufficiently, and it will not boil till this be done. But if we hold the end B a little down, ſo that ſome water may run into it from A, and thus leave a part of A empty, and then, holding the inſtrument level, if we graſp A in the hand, the heat is ſufficient for immediately converting into vapour the thin film of water which now lines the empty part of A. The vapour expands, puſhes all the water out of A into B, and then ruſhes up through it, throwing it into the agitation called boiling; and it is a real boiling, although with a warmth that is imperceptible. On the contrary, the ball A feels even cool to the hand. If we allow all the water to come back again, except ſo as to have a very ſmall bubble in the upper part of A, and now apply the tongue to that part, we ſhall immediately throw it into a violent ebullition in the ball B, and A will feel very cold to the tongue. The ebullition and the cold will continue till all the film of water which lined the inſide of A is completely evaporated, and it has become perfectly dry. The boiling in B now ceaſes, and, with it, the coldneſs of A. Nothing can ſhew, in a more convincing manner, the prodigious quantity of heat abſorbed by water, in the act of its converſion into vapour, than the coldneſs produced, during a very conſiderable ſpace of time, in order to evaporate this trifling film of water.

It is this great quantity of heat contained in ſteam that makes it ſo powerful and effectual in the buſineſs of cookery. The knowledge now acquired of the latent heat of ſteam, has, of late years, been applied by ingenious artiſts to this purpoſe, with very great effect, giving to all the operations of the kitchen, where boiling or ſtewing is concerned, a neatneſs and eaſineſs of management that is very remarkable; and this is attended by a great ſaving of fuel. Whoever attends to the

manner in which heat is applied, in our kitchens, to all cooking veſſels, will be convinced that not leſs than three-fourths of our fuel is waſted, in the production of a heat which is extremely troubleſome to the cook, and, after all, goes up the chimney, without doing any ſervice. A great ſurface of fuel, in a ſtate of incandeſcence, heats a great quantity of air, both by radiation and by material communication. We ſet a boiler on this open fire, in the midſt of a copious ſtream of heated air, which is inceſſantly ruſhing up the chimney. Only a very ſmall quantity of this comes in contact with our boiler. The bottom of it is, indeed, heated by the fuel alſo on which it reſts; but the greateſt part of all the heat produced is running to waſte. If, inſtead of ſetting the veſſel on the open fire, we ſet it on a well conſtructed furnace, where the heated air ſhall all be forced to come in contact with its bottom and ſides, we are certain that more than nine-tenths of the fuel may be employed in communicating heat to water boiling in the veſſel, the remaining tenth being expended in maintaining the heat of the furnace, or eſcaping with the air by the flue. By this operation, the water is converted into elaſtic vapour, furniſhed with its latent heat, which is ready to emerge and affect any body that is colder than 212°. It would, therefore, very quickly heat the water in another ſimilar veſſel up to the ſame temperature, and would keep it in that ſtate, although it cannot cauſe it to boil. And it will do this in a manner far ſuperior to the hot air of a fire; for this laſt acts only by the film, which is in perfect contact with the veſſel; and the hot air, which is half an inch farther off, paſſes by, without doing the leaſt ſervice. But, if the ſteam from our boiler be admitted into a box, already heated to 212°, and containing a piece of meat, or a veſſel which contains any thing to be dreſſed by a boiling heat, not a particle of vapour will paſs by it, but will all come and depoſit on it the whole of its latent heat. And this will continue without interruption, till the veſſel, with its contents, have attained the temperature 212°. Then, and not till then, will the ſteam fill the box, and blow through any

hole made in its top or ſides. This hole may communicate with another box. The ſteam, *having nothing left it to do* in the firſt, will go into the ſecond, where it will be equally effective. Steam is the moſt faithful carrier of heat that can be conceived, and will depoſit it only on ſuch bodies as are colder than boiling water.

In conſtructing an apparatus for this purpoſe, the moſt adviſeable method ſeems to be, to convey the vapour from the boiler into a long wooden box, divided by partitions into portions fit for holding the different veſſels of cookery, and always to employ thoſe which are neareſt to the boiler. If any thing be dreſſing in a remote diviſion, and we put a cold veſſel into an intermediate one, the operation in all the diviſions beyond it will be ſuſpended, till the new veſſel be boiling hot; and the others will now be cooling, and we muſt expend ſteam in order to reſtore their proper heat. Wooden boxes are recommended, becauſe they will waſte leſs heat by communication to the air; but they muſt be lined with thin metal, otherwiſe they will ſoon be deſtroyed by the ſteam.

Employed as we are juſt now, in the conſideration of the production of elaſtic vapour, and its condenſation into water again, it would be unpardonable to omit taking notice of the application which has been made of theſe properties to the arts of life, by the invention of the STEAM ENGINE; an invention, which, I may truly ſay, is, in its preſent ſtate, the maſter-piece of human ſkill. Nor was it, like the pump, the mariner's compaſs, the teleſcope, the production of a chance obſervation, but the reſult of deep thought and reflection, and really a preſent by philoſophy to the arts.

The power of heat to increaſe the elaſticity of ſteam to the moſt enormous degree, had been obſerved by many, and Dr. Papin had made an ingenious uſe of the heat accompanying its great compreſſion in his digeſter. The Marquis of Worceſter, a gentleman of extenſive natural knowledge, and moſt inventive mind, obſerved farther, that ſteam, when exerting this enormous force, could be, in an in-

ſtant, deprived of it, by daſhing a little cold water through it, and that a vacuum would thus be produced in the veſſel, which the moment before was ready to burſt. He therefore, about the year 1660, contrived, moſt ingeniouſly, to make this veſſel V (fig. 4.) communicate alternately with a boiler B, from which it ſhould receive, through a cock S, this highly elaſtic ſteam, and with a ciſtern C, from which it was required to raiſe or force the water. The veſſel being ſuppoſed to be filled at firſt with common air, the ſteam was admitted, and it quickly blew out all the air. The ſteam-cock being now ſhut, a jet of cold water was admitted by another cock I, which, in a moment, condenſed the ſteam, and made a vacuum, and water ruſhed in from the ciſtern and filled the veſſel. The condenſing cock being now ſhut, the ſteam-cock was again opened; ſtrong ſteam was admitted, which by its violent preſſure on the ſurface of the water, drove it out by another pipe EF, which conveyed it to where it was wanted.

This moſt curious contrivance was immediately put in practice, and excited the wonder of every perſon in that moſt ardent and inquiſitive age. The engine was found, however, ſo limited in its practicable powers, and ſo difficult, and even hazardous to manage, that it never came into very general uſe. But, about the beginning of this century, its principle and form underwent a great revolution. The great elaſticity, which made it ſo hazardous, was found unneceſſary; and the ſteam, no ſtronger than common air, was employed merely as a mean of rapidly making a vacuum in a veſſel by its condenſation. The contrivance was as follows:

The ſteam-veſſel V, (fig. 5) is now a metal cylinder, ſmoothly bored, communicating with the boiler B, through the ſteam-cock S, and with the cold water ciſtern A, through the injection-cock I. A great piſton, P, exactly fitting the cylinder, hangs by one end of the working beam DH, which turns round an axis or gudgeon, G. From the other end of this beam, hangs the piſton Q of the pump, which draws the water from the pit. This end of the beam is made

to preponderate, ſo that, when the great piſton P is at the bottom of the cylinder, the weight hanging on H ſhall be able to overcome all the friſtions, and to drag the great piſton to the top of the cylinder. The ſketch, therefore repreſents the engine in its natural inactive poſition.

The water being made to boil ſtrongly in the boiler, the cock S is opened, and the ſteam ruſhes into the cylinder, and drives all the air out of it by the pipe E near the bottom. When the attendant obſerves the ſteam following in abundance, he ſhuts the ſteam-cock, and opens the injection-cock I. Cold water now ſpouts up into the cylinder, daſhes againſt the piſton P, and, ſcattering over the whole veſſel, immediately condenſes the ſteam. The preſſure of the atmoſphere ſhuts the valve lying on the mouth of the ſnifting-pipe E; and a vacuum is now produced in the cylinder. The atmoſphere preſſes on the upper ſurface of the great piſton, with a force of nearly 15 pounds on every inch, or a ton on every ſquare foot. This forces down the great piſton P, and draws up the piſton Q with the column of pit-water correſponding to it. When the piſton has reached the bottom of the cylinder, the attendant ſhuts the injection-cock, and opens the ſteam-cock. The ſteam, being as elaſtic as common air, performs the ſame office that the opening of the valve E would do, that is, it allows the preponderancy at H to drag the great piſton to the top of the cylinder, where it is again ready to receive the preſſure of the atmoſphere, and make a ſecond working ſtroke.

It is plain that this conſtruſtion has totally changed the engine, both in form and in principle. We no longer need a highly elaſtic ſteam, in order to raiſe a great column of water. We need only to enlarge the diameter of the cylinder, till the preſſure of the atmoſphere ſupplies a force equal to the taſk. A cylinder of 30 inches diameter ſupplies a force of five tons. It is a vulgar miſtake, that the prodigious power of this engine is owing to a great force in the

ſteam. Its only uſe is the rapid production of a vacuum, by the condenſation produced by the injection of cold water.

Such is Newcomen's engine. By its aſſiſtance, many mines have been continued, and many have been opened and worked, with great advantage to the proprietors and to the public, which muſt have otherwiſe been abandoned, notwithſtanding the earth was full of treaſure; and the abundance of pit-coal procured, by theſe means, to theſe kingdoms, gave it a ſuperiority in many manufactures, which has been of unſpeakable advantage to this nation.

I have the pleaſure of thinking, that the knowledge which we have acquired concerning the nature of elaſtic vapour, in conſequence of my fortunate obſervation of what happens in its formation and condenſation, has contributed, in no inconſiderable degree, to the public good, by ſuggeſting to my friend Mr. Watt of Birmingham, then of Glaſgow, his improvements on this powerful engine. The great hindrance to its employment, for every purpoſe, not only of mining, but even of manufactures, by giving us a moving power of unlimited force, has been the prodigious expence of fuel. An engine, having a cylinder of three feet diameter, and working continually, conſumes about 2000 chaldron (London meaſure) of coals in a year, or almoſt 3000 tons *. Many attempts have been made by engineers to diminiſh this enormous expence; but no way occurred to them but improving the conſtruction of their furnaces, and by preventing all waſte of heat that could be avoided. The flame was made to circulate round the boilers; the fire was placed in the middle of the boiler; the machine was carefully defended from the air, in all places where its heat was great. Mr. Watt alſo directed his attention to theſe things; but he ſoon found that all theſe were but trifling improve-

* 100 pounds weight of the beſt Newcaſtle coal, when applied by the moſt judiciouſly conſtructed furnace, will convert about 1½ wine hogſheads of water into ſteam, that ſupports the preſſure of the atmoſphere.

ments, and that the copious production of ſteam was not enough, unleſs it was afterwards huſbanded with the moſt rigid economy. He ſaw that more heat was waſted after the ſteam was produced, in conſequence of its being miſmanaged, than by the moſt injudicious conſtruction of furnaces. For, as ſteam contains ſuch a great quantity of heat, a waſte of ſteam is a ſure waſte of heat. As much heat is contained in one gallon of water in the form of ſteam, as would bring $5\frac{1}{2}$ gallons of ice-cold water to a boiling heat.

In Newcomen's engine, a vaſt quantity of ſteam is waſted by unavoidable condenſation, the moment it enters the cylinder; for the cylinder has been cooled down to 100°, or 110°, by the injection; and there is lying at the bottom a quantity of water, perhaps even colder than this. It is alſo cooled by the deſcent of the piſton, covered with ſeveral inches of cold water. All this muſt be heated again to the temperature 212°, before the piſton begin to riſe; and, as it goes up, more of the cooled ſurface of the cylinder is laid open to the ſteam, and condenſes it; and the piſton cannot reach the top till the whole cylinder be again raiſed to 212°. Then comes the injection, which again cools every thing. It is plain that this waſte will be increaſed by injecting too much cold water; therefore the engineers were careful not to do this. But they blundered as much by injecting too little; for this quantity of water, not being ſufficient to abſorb all the latent heat of the ſteam, was raiſed to a conſiderable temperature, and therefore allowed a ſteam to remain, whoſe elaſticity bore a very ſenſible proportion to the preſſure of the atmoſphere; and this balanced that portion of the preſſure, and rendered the piſton unable for the full load of 15 pounds on every ſquare inch. To preſerve this power, requires the injection of an ale pint of cold water, for every cubic inch of water that has been converted into ſteam, or for every cubic foot of ſteam condenſed. Mr. Watt imagined that half of the ſteam produced in the boiler was thus waſted.

But how was this to be remedied? The waſte is unavoidable, while

the ſteam-veſſel is thus cooled by every injection; therefore it muſt be waſted as long as the condenſation is performed in the cylinder, or ſteam veſſel. He therefore contrived to perform it in another veſſel, in the ſame way as it is done in the common proceſs of diſtillation. His condenſer is a ſeparate veſſel, occaſionally communicating with the great cylinder. He knew that ſteam, being an elaſtic fluid, will go into any communicating veſſel, if emptied of its air, or other contents. And if this condenſing veſſel be kept extremely cold, by being immerſed in cold water, or any other method, he preſumed that it would all be condenſed there. But all this is not enough. The cylinder will ſtill be cooled by the deſcent of the piſton, and the cold water above it. Mr. Watt, therefore, did not allow the external air to preſs down the piſton, but employed the hot ſteam from the boiler, which is equally powerful, its elaſticity being already a balance for the preſſure of the atmoſphere: It will, therefore, preſs as ſtrongly on the upper ſurface of the piſton. Thus is the cylinder kept continually hot,—as hot as boiling water; and it may be incloſed in a caſe, to prevent loſs of heat from ſo hot a ſurface of great extent.

Mr. Watt's conſtruction is, therefore, as follows: The ſteam-veſſel D (fig. 6.) communicates with the boiler F, by the pipe A A leading from its top, and having a ſtop-cock A. It communicates at the bottom with the condenſer E, which is immerſed in a ciſtern of cold water. The pipe of communication has a ſtop-cock B. A communication is alſo eſtabliſhed between the upper and under parts of the ſteam-veſſel, by means of the pipe C C, alſo furniſhed with a ſtop-cock C. Both D and E are neatly bored, and have piſtons, which move ſmoothly up and down, without allowing any air, or other fluid, to paſs by their ſides. Both piſtons are furniſhed with ſhanks, which move air-tight, in holes made in the covers of the cylinders. The quieſcent poſition of the engine is ſuch, that the piſtons are at the top of the cylinders D and E, becauſe they hang from the inner end of the great beam, and the outer end of it is made to

preponderate a little, ſo as to drag both piſtons to the top of the cylinders.

In ſetting the engine to work, all the cocks are opened. The ſteam, paſſing through the cock A, gets into the top of the cylinder, and, from thence, along the pipe C, into the bottom of the cylinder, and fills it, driving out the air through the cock B into the condenſer, and then through the valve of the condenſer-piſton. This being done, the cock C is ſhut, and now, as the ſteam is condenſing rapidly in the condenſer, it is rarefied in the cylinder below the piſton, while it preſſes on the upper ſurface of it with its full force. It therefore preſſes down the piſton, almoſt to the bottom, but not altogether, by reaſon of ſome air remaining in it. When the piſton is at the bottom of the cylinder, the cocks A and B are ſhut, and C is opened. Thus, by eſtabliſhing the communication between the top and bottom of the cylinder, the ſteam preſſes equally on the upper and under ſurfaces of the piſton, and now the preponderancy of the outer end of the beam is permitted to drag the piſton to the top again; and the ſteam, which was formerly all above it, is now all below it, having come round by the cock C. This cock is now ſhut, and A and B are opened. There is, at preſent, a vacuum in the refrigeratory E, by the riſe of its piſton. The ſteam, therefore, in the cylinder expands into this empty ſpace, and, as faſt as it enters, it is condenſed by its cold ſides, and falls to the bottom, in the form of water. A vacuum being thus made below the piſton, it is preſſed down by the ſteam, even although this ſteam were no ſtronger than common air. It preſſes down with a force equal to the ſum of the preſſure of the atmoſphere, and the exceſs of its elaſticity above that preſſure. The water produced in the refrigeratory, by the condenſation of the ſteam, and the ſmall quantity of air which is extricated from it, or has entered by crevices, or remained after the firſt deſcent of the piſton, is extracted at the next upward movement of the piſton of the condenſer.

By this conſtruction, the cylinder is kept boiling hot, and waſtes

no ſteam. The vacuum alſo, made by the condenſation, may be as perfect as we pleaſe, becauſe there is no occaſion to be ſparing of cold water for the condenſation. This circumſtance gives a great mechanical ſuperiority to this conſtruction. For it has been found by long experience, that in the old engine, it is adviſeable to work againſt no greater load than ſix or ſeven pounds on every ſquare inch of the piſton, becauſe the quantity of cold water that muſt be employed, for producing a greater unbalanced preſſure on the piſton, cools the cylinder ſo much, that more loſs is ſuſtained by the expence of fuel to replace the waſted ſteam, than is gained by the increaſe of power. In Mr. Watt's conſtruction, the engine will work very tolerably againſt a load of $13\frac{1}{2}$ pounds, and is never loaded with leſs than $11\frac{1}{2}$; but its moſt conſpicuous advantage is, that an engine of this conſtruction conſumes not more, and generally a good deal leſs, than one third of the fuel uſed in the very beſt common engine of the ſame ſize.

Mr. Watt ſtill farther improved this engine, by making it exert an active ſtroke in both directions. This was very obvious and eaſy. He had only to make both the upper and under parts of the cylinder communicate with the boiler and condenſer. When the upper part communicates with the boiler, and the lower with the condenſer, the piſton is ſtrongly preſſed down; and when theſe communications are reverſed, it is as ſtrongly preſſed up. Moreover, by an artful cutting off, or greatly diminiſhing the ſupply of ſteam, before the piſton has completed its movement, he has enabled a given quantity of ſteam to produce a much greater effect, and at the ſame time has brought the engine ſo much, and ſo inſtantaneouſly, under command, that it is really more manageable than the exertion of any animal. But the explanation of theſe particulars is not in our department, but that of the mechanician.

I muſt not, however, omit an important chemical obſervation which occurred to Mr. Watt in the courſe of thoſe improvements, and particularly in the proſecution of a project which he had formed

for diſtilling *in vacuo*. This is a project which I have thought of ever ſince I ſaw the curious experiments of Dr. Cullen, already related to you *. In theſe it was plain, that when the preſſure of the air is removed, volatile liquors will boil, and therefore diſtil, at very low heats. Mr. Robiſon's experiments, already mentioned, alſo ſhew that water will certainly diſtil at the temperature 90°, and vinous ſpirits at the temperature 55°. I ſee that Fontana has had the ſame project; and indeed it is very obvious. Mr. Watt, having carefully examined the elaſticity of watery vapours, in a variety of temperatures, between 32° and 212°, concluded that water would paſs copiouſly by diſtillation, when of the temperature 70°, if the refrigeratory be kept in melting ſnow; or, that it will diſtil equally well of the temperature 90° or 95°, in the ordinary temperature of this climate.

He accordingly made the experiment, and it ſucceded perfectly well. A very ſmall ſtill was half filled with water, and then cloſely united with the veſſel which was to receive the diſtilled water. A very ſmall hole was made in the bottom of the receiver, and a plug was fitted to it. The water being made to boil violently, ſent the ſteam through the whole apparatus, (there being no water in the refrigeratory to condenſe it) forcing the air out by the hole. While the whole was boiling hot, and the ſteam blowing through the hole, this was ſuddenly ſtopped up by the plug, and the bottom of the ſtill was ſet on ice. This ſoon cooled its contents; and the ſteam, which occupied the reſt of the apparatus, collapſed into a few drops of water. A lamp was now ſet under the ſtill, and in a few minutes the whole apparatus grew warm: a proof that ſteam was now produced from the water, and paſſed over into the receiver. Cold water was put into the refrigeratory, and the diſtillation went on; ſlowly indeed, but very well; and the ebullition was very diſtinctly heard and felt in the ſtill, although the head of it was ſcarcely warm to the hand. But the reſult of the proceſs was, that the latent heat of the ſteam was greatly

* I ſee this project in a note-book written before 1757. EDITOR.

increaſed by this diminution of its ſenſible heat. The temperature of the ſteam in the experiment was found to be 100. The water in the refrigeratory was raiſed by the condenſation of the ſteam, from 57° to 77°, and the veſſel had acquired as much heat as could have raiſed the water one degree. Therefore, 21 degrees of heat had been acquired from the ſteam. The quantity of water diſtilled was $\frac{1}{51}$ of that in the refrigeratory; therefore, the increaſe of temperature, namely 21°, multiplied by 51, will give the heat extricated from the ſteam. This is 1071. From this take 100-77, or 23, the ſenſible heat loſt by the ſteam, and there remains 1048 for the latent heat of this ſteam.

Mr. Watt has made other experiments, with much more care, and in various ſtates of heat and elaſticity of the ſteam. He finds that water diſtils perfectly well, when of the temperature 70°, and that, in this ſtate, the latent heat of the ſteam approaches to 1300, and certainly exceeds 1200. The unexpected reſult of theſe experiments, is, that there is no advantage to be expected, in the manufacture of ardent ſpirits by diſtilling *in vacuo*. For we find that the latent heat of the ſteam is at leaſt as much increaſed as the ſenſible heat is diminiſhed. This will undoubtedly be attended with an increaſed expenditure of fuel. For the increaſe of 100 degrees of ſenſible heat occaſions an increaſe of fuel, only while we are raiſing the temperature of the ſtill to the ordinary heat of boiling water, in the beginning of the diſtillation. If the furnace be judiciouſly conſtructed, and due precautions taken to prevent diſſipation, it requires very little fuel to maintain this temperature. But 100 degrees of latent heat is an expence that is continual, and which no contrivance whatever can prevent.

This train of experiments ſuggeſted to Mr. Watt many very curious and important reflections concerning the combinations of bodies with heat; and his opinions on this ſubject have been farther eſtabliſhed, by the whole courſe of his experience, in the conſtruction and management of his double-ſtroke ſteam engine.

We are now much more able to underſtand the nature of vapour than formerly, and to trace ſome of the ſteps by which it is produced. It is ſurely an approach to the explanation of any phenomenon in nature, when we obtain certain information of circumſtances indiſpenſably neceſſary for its production; and the clearneſs or competency of this explanation is greatly augmented, when thoſe circumſtances are of a meaſurable kind, and invariably connected with certain gradations in the phenomenon itſelf. We ſeem to have attained this deſirable point in the preſent caſe, and in that of liquefaction and congelation. There is a ſomething to which I have given the familiar name of heat, which is ſuſceptible of degrees. It not only gives us the ſenſation of greater and leſs warmth, or heat, but it alſo produces other effects, which are equally variable and diſtinguiſhable in quantity. We might take any of theſe variations for the meaſures of its degrees, if we find them always conſiſtent. We have not been able to meaſure our feelings of heat with accuracy, and we even obſerve, that the moſt accurate meaſures of them would not be accurate meaſures of their cauſe; becauſe, in caſes where we know that the general cauſe is the ſame, the degree of the ſenſation may be different. The expanſion of certain bodies has been found a very good meaſure, merely becauſe, in caſes in which we know, *by other proofs*, that the cauſe is the ſame, or double, or triple, the expanſion is alſo the ſame, or double, or triple. But, in the application of this meaſure to our purpoſes, we met with two very remarkable irregularities, which, had they occurred earlier, might have hindered us from adopting expanſion as a meaſure of heat, and deprived us of the thermometer,—I mean the paſſage of a body from ſolidity to a liquid form, and from this to the form of vapour. Had we only examined bodies which change their ſtate gradually, ſuch as ſealing wax, we ſhould perhaps have formed very different notions of the ſcale of heat, after which we ſhould have been in continual miſtakes, while conſidering the heats or temperatures of ſuch bodies as water, lead, mercury, &c. But the

obſervations I have laid before you have removed all thoſe difficulties, and have enabled us to include even thoſe great anomalies in our general ſcale; ſo that we continue to have the ſame confidence as before, in the ſcale of the thermometer, as a ſcale of the differences of heat. We have learned, that when ice melts, or when water boils, the cauſe of the phenomenon is the ſame with that of expanſion; becauſe we obſerve, that what produces a double or triple expanſion, alſo produces a double or triple liquefaction, or vaporiſation; and that congelation and condenſation accompany contraction, which is always produced by the abſtraction of what was the cauſe of expanſion. But we ſee reaſon to think, that the manner in which the general cauſe of all theſe phenomena acts, in producing its effects, is ſomehow different. We find, that while the action of heat expands a body, it does not melt it; and when it melts it, it does not expand it. If we conceive heat to be a ſubſtance *ſui generis*, ſuſceptible of union or combination with bodies, ſimilar or analogous to thoſe combinations which the chemiſt obſerves between numberleſs varieties of ſubſtances, we muſt conclude that the union which produces expanſion is ſomehow different from that which produces liquefaction, &c.

When we perceive that what we call heat diſappears in the liquefaction of ice, and reappears in the congelation of water, and a number of analogous phenomena, we can hardly avoid thinking it a ſubſtance, which may be united with the particles of water, in the ſame manner as the particles of Glauber's ſalt are united with them in ſolution, and may be ſeparated as theſe are. But, ſince heat has never been obſerved by us in a ſeparate ſtate, all our notions of this union muſt be hypothetical. Moreover, this hypotheſis muſt be combined with ſome other hypotheſis, which we may have adopted concerning the unions of thoſe other ſubſtances; for it muſt be acknowledged, that our notions of thoſe more palpable and familiar combinations are all hypothetical. We think that we can conceive how a particle of common ſalt can draw around it, and attach to itſelf, particles of wa-

ter, and how they may adhere, and compoſe a ſaline cryſtal.—We transfer this notion to the relation between bodies and heat, and (without much reflection, or diſtinct conception) we ſuppoſe, that, in like manner, a particle of a body attaches itſelf to a number of particles of heat. Heat is, therefore, ſuppoſed to be ſomehow contained or lodged in the pores of bodies, and we endeavour to account for the changes of ſenſible appearance, ſuch as increaſe of bulk, or fuſion, or vaporiſation, &c. by ſhewing ſome reſemblance between thoſe appearances, and thoſe which occur in chemical unions or mixtures.

Many have been the ſpeculations and views of ingenious men about this union of bodies with heat. But, as they are all hypothetical, and as the hypotheſis is of the moſt complicated nature, being in fact a hypothetical application of another hypotheſis, I cannot hope for much uſeful information by attending to it. A nice adaptation of conditions will make almoſt any hypotheſis agree with the phenomena. This will pleaſe the imagination, but does not advance our knowledge. I therefore avoid ſuch ſpeculations, as taking up time which may be better employed in learning more of the general laws of chemical operations. I content myſelf with ſaying, that in liquefaction and vaporiſation, water abſorbs a great quantity of heat, becauſe this expreſſion immediately raiſes the notion of a ſudden, and ſomewhat copious, accumulation of heat. And I ſay, that this great quantity of accumulated heat is *latent* in the water, or in the vapour,—merely becauſe the thermometer, our uſual teſt, does not give us any indication of its preſence; of which preſence we are not allowed to doubt, when we ſee the ſame quantity emerge again, in its uſual thermometrical activity, when the water freezes, or the vapour collapſes. Without ſaying that I have any clear conception of the union between bodies and heat, I am well entitled, by the phenomena, to ſay, that this vaporous combination differs in ſome peculiar manner from that which merely produces expanſion. And, becauſe I find that vapour cannot exiſt without this latent heat, any more than aquafortis can

exiſt without nitrous acid, I conſider latent heat as conſtituting a part of aqueous vapour.

Some chemiſts think that the phenomenon of the production of elaſtic vapour is brought about in a different way. Almoſt immediately after my diſcovery of this remarkable abſorption and concealment of heat, in the phenomena of fuſion and ebullition, Dr. Irvin, whoſe ſpeculations were always ingenious, conceived that there was no occaſion for the diſtinction between ſenſible and latent heat, in the peculiar circumſtances of theſe phenomena. He imagined, that in every caſe there is a portion of heat which does not affect the thermometer; and that the reaſon why there is a ſeeming diſappearance in liquefaction and ebullition is, that when the element changes its form from that of water to that of vapour, its capacity for heat is ſuddenly increaſed; and, conſequently, it requires much more heat to be thrown into it, in order to make it appear equally hot by the thermometer. If it were in our power, by a wiſh, to convert an ounce of water, of the temperature 50°, for example, into vapour, without adding heat to it; this vapour, if examined immediately by a thermometer, would appear exceſſively cold, becauſe the heat already in this ounce of water is not ſufficient to heat an ounce of vapour to this temperature;—its heat is that of an ounce of vapour of an incomparably lower temperature; and it would therefore affect the thermometer as ſuch, that is, would rob it of its heat, or cauſe the liquor to contract. The vapour has a much greater capacity for heat than the water from which it was produced. Heat will, therefore, flow into it from the ſurrounding bodies, till they are reduced to an equilibrium of temperature. And, on the contrary, when the capacity of the vapour is diminiſhed, for example, by violent compreſſion, and it is changed into water, this heat is redundant; the ſubſtance has not capacity for retaining it; it muſt be thrown out, as water is ſqueezed out of a ſponge.

This is an ingenious thought, and gives a very ſimple and intelli-

gible view of ſome of the appearances: But it does not aſſign a cauſe for the formation of vapour. The abſorption or diſappearance of the heat is here conſidered as the *conſequence* of the formation. The vaporous form muſt precede the abſorption, if a greater capacity for reception of heat be the cauſe of this abſorption.

Although this attempt to explanation, by an increaſe of capacity ſeems to have given very general ſatisfaction, I acknowledge that it does not appear to me to be any thing more than a different expreſſion for what I have called abſorption. There was ſurely ſomething in the water, which made it unſuſceptible of more heat, without riſing in its temperature, and that ſomething is now removed, and the water is now capable of receiving more heat without ſharing it with a thermometer. But, if by capacity we are to underſtand more ſpace in which heat may be lodged, I apprehend that we are ſpeaking without ſufficient authority. The ſame explanation is given of liquefaction, although it be well known that many ſubſtances occupy leſs room in their fluid than in their ſolid ſtate *.

Next to Dr. Irvin, the ingenious Dr. William Cleghorn applied his peculiar theory to the ſame ſubject, and ſhewed that it is remarkably adapted to it. This theory is pretty much the ſame with the opinion generally entertained by the modern dynamical philoſophers, as they are called, on the Continent, who conſider heat as conſiſting of particles mutually repellent, and ſtrongly attracted by the particles of all bodies. The different kinds of matter will attach to them different proportions of this ſubſtance, in conſequence of the different ſtrength with which a particle of each ſubſtance attracts a particle of heat. The chief limit to the quantity attracted is this repellent power between the particles, by which it becomes a ſort of elaſtic fluid. The more that its particles are conſtipated by the attraction of the particle of other matter which accumu-

* I would aſk the partiſans of this doctrine of Capacity how it will explain the burſting of veſſels by ſteam?

EDITOR.

lates them round it, the more active muſt their mutual repulſion be. And thus, when the attractive force of any ſubſtance for heat is given, the quantity that can be conſtipated by it in a given ſpace is limited. Each body, while it receives heat, is ſuppoſed to attract and condenſe more or leſs of theſe mutually repelling particles, according to its ſtrength, and when this is in equilibrio with the whole of their mutual repulſion, no more will be collected from the ſurrounding bodies, which have alſo their reſpective attraction for them. But when this equilibrium is attained, in the caſe of water, for example, if we ſuppoſe it ſuddenly converted into vapour, the particles of heat in it, having now much more room, are much leſs conſtipated round any one particle of the vapour; therefore, the repulſion of one of the particles, for the others ſurrounding that particle of vapour, muſt be greatly diminiſhed, by their removal to a greater diſtance. But the watery particle, having ſtill the ſame, or nearly the ſame attraction for heat as before, can now accumulate many more around itſelf, before their mutual repellency balances its attraction. They will, therefore, be drawn from the neighbouring bodies, and while this goes on, the vapour muſt appear colder than them. Dr. Cleghorn adduces, in proof of this theory, the well known fact, that a thermometer, in the receiver of an air-pump, deſcends when the air is rarefied, and riſes when it is condenſed.

There is no doubt but that this ſuppoſition agrees pretty well with this ſingle phenomenon. We can form a pretty diſtinct notion of it; but, like the former, it gives no explanation of it. It does not ſhew us how the gathering a certain quantity of heat round the particles of water ſhould be followed by an immediate and great ſeparation of thoſe particles from each other, each carrying with it its due proportion of calorific matter, ſo as to leave room for the reception of a great deal more: Far leſs will it ſhew us how the abſtraction of the ſmalleſt portion of this additional heat by a colder body, inſtantly brings all the particles of the vapour together, again expelling the heat. An

active cauſe for the change of form is wanted, before we can ſpeak either of enlarged capacity, or leſs repulſion.

Many other ſtrong objections may be made to this explanation. Some of theſe will occur when we conſider the nature of chemical union; and it is needleſs to dwell more on this ſpeculation, becauſe it gives very little addition to our chemical knowledge. I content myſelf with ſaying, that it is much more reaſonable to conclude, that the abſorption and entrance of this great quantity of heat into the compoſition of vapour is the cauſe of its formation, than that it is its conſequence; becauſe we have the greateſt reaſon to be ſatisfied that heat, in one ſhape or other, is the principal or only cauſe of the formation of vapour, as it is of fluidity *. *(See Note 5. at the end of the Volume.)*

I have already obſerved, that the heats which produce vapour from bodies under the preſſure of the atmoſphere, are, with regard to the greateſt number of them, higher than thoſe which produce ſimple fluidity. This is, however, not the caſe with reſpect to all kinds of bodies. There are ſome ſolid ſubſtances, which, when we heat them, evaporate entirely, or are totally changed into vapour, without previouſly melting, or ſhewing any tendency to become ſimply fluid; and the vapour into which they are changed, when cooled again, condenſes into ſolid matter, not into a fluid. Such are camphor, the volatile alkali, ſal ammoniac, arſenic, corroſive ſublimate of mercury, and ſome others. Theſe bodies, however, are capable of being brought into a melted ſtate, by increaſing the preſſure on their ſurface above that which they commonly ſuſtain from the weight of the atmoſphere, which muſt be done by confining them and their ſteams in cloſe veſſels. The conſequence of this is, that they are forced to bear more heat, and ſeveral of them can thus be made ſo hot as to

* See Lavoiſier's Mem. 1777, in which he explains his notion of the manner in which all elaſtic fluids are formed of ſome ſolid matter, and the matter of heat.

become perfectly fluid. This ſhews, therefore, that the principal difference between them and other bodies is, that whereas other bodies are more eaſily melted than changed into vapour by heat, under the ordinary preſſure of the air, theſe, on the contrary, are ſooner changed into vapour than melted, and ſome of them ſo much ſooner, or with ſo much leſs heat, that we dare not venture to confine them ſo long as is neceſſary to learn what degree of heat is requiſite for their fuſion. They are ſo volatile that they would burſt any veſſels in which we could attempt to make the experiment.

Theſe differences of bodies from one another will enable you to underſtand the nature of ſeveral chemical operations, which depend upon the production of vapour; ſuch as *evaporation*, *diſtillation*, and *ſublimation*. Theſe are operations occaſionally performed with mixtures of bodies, or compound ſubſtances, the different ingredients of which we deſire to ſeparate from one another; and they can only be performed with thoſe mixtures or compounds which contain ingredients differing in volatility, and which are not united together by a ſtrong attraction. When this is the caſe, all that is neceſſary is to expoſe the whole to heat, very gradually increaſed, ſo that the more volatile may be gradually converted into vapour, and wholly ſeparated, before the more fixed. It generally happens that the ſeparation effected in this way is not complete. The action of heat in the converſion of a body into vapour, ſeems not to deſtroy altogether the former combination. A very ſcanty ſupply of heat promotes the ſeparation, ſeeming to allow the difference of volatility to produce its effect with greater certainty. This however ſeems a difficulty. An operation of this kind is called evaporation, when performed with a view to obtain the more fixed parts, and when we are careleſs about the more volatile, and therefore expoſe the matter to heat in open veſſels, and allow the volatile matter to fly away in the air, as in the manufacture of common ſalt: But in diſtillation and ſublimation, we preſerve the volatile parts, which is done by performing the operation in veſſels

and inſtruments, by which the vapours are not allowed to eſcape, but are directed into a part of the apparatus in which they are cooled and condenſed again. In this, diſtillation and ſublimation agree, and the diſtinction between them depends on the ſtate and appearance of the condenſed matter produced from the vapour. In diſtillation, this condenſed matter is a fluid; in ſublimation, it is a ſolid ſubſtance. In one of theſe operations, therefore, we work upon ſubjects that are more fuſible than volatile, or more eaſily melted than volatilized; in the other we work upon thoſe that are more volatile than fuſible.

There are ſeveral other terms which might be taken notice of here, but it is not to be ſuppoſed that you would remember them, though I ſhould enumerate and explain them at preſent. I ſhall have occaſion to notice them hereafter.

Beſide thus explaining theſe chemical operations, the doctrine of vapour will enable you to underſtand many particular effects which heat produces on certain bodies of a compound kind; ſuch as entirely changing their appearance and properties,—converting them, as it were, into new ſubſtances. This happens, becauſe ſuch compound bodies conſiſt of ingredients which differ in volatility, and cohere weakly. Heat diſſipates ſome in the form of vapour; and both the volatile and more fixed ingredients combine in a new manner. Hence wood contracts, inſtead of expanding; and inſtead of melting afterwards, is converted into water, oil, ſalt, charcoal, earth. And clay contracts always the more it is heated, and remains ever after in the contracted ſtate.

Nothing general can be delivered on this head, becauſe theſe are all inſtances of peculiar properties. We can only ſee clearly, that ſubſtances, which are brought into particular ſtates of combination by the operation of nature or art, may continue in that ſtate unchanged, while their temperature remains the ſame. But when we raiſe this to a degree which volatilizes ſeveral of their ingredients, and when theſe are ſuch as will unite, either in this high temperature, or in a lower,

when each has collapſed, it muſt happen that we ſhall produce compounds which did not exiſt in the body from which they have been raiſed. And it may happen, that the ingredients that are left behind, are ſuch as will unite, but had taken another combination, by the intervention of the ingredients now volatilized. We ſhall thus obtain other compounds which did not exiſt in the body.

Theſe reſults appeared very wonderful, while the ſcience of chemiſtry was in its infancy, and led into many miſtakes as to the compoſitions of bodies. The detection of thoſe miſtakes, conſtitutes the chief part of the advances which have been made in the ſcience to this day, and will occupy the greateſt part of the following courſe of lectures.

It remains now to be conſidered, whether this effect of heat may be ſuppoſed univerſal with regard to all bodies, or whether there are any exempted from this power of heat. Many philoſophers are inclined to ſuppoſe that it would be found univerſal, were it in our power to employ heat of the utmoſt intenſity; and we have, no doubt, very extenſive experience of the poſſibility of converting into vapour a great many of the different kinds of matter, many of them ſuch as might not at the firſt view ſeem much diſpoſed to undergo this change; ſuch as all the different kinds of ſalts, and the inflammable ſubſtances, and the metals. But ſtill we find much difficulty with reſpect to the earthy bodies. The greater number of theſe ſhew no diſpoſition to volatility, but, on the contrary, reſiſt the action of our moſt violent fires, without any ſigns of their being changed into vapour. There are, however, one or two of them that, in very violent heats, are more or leſs diſſipated; and from theſe examples, ſome may perhaps imagine that the reſt might likewiſe be volatilized, were it in our power to command and employ more violent heats; but this is only a ſuppoſition.

Some philoſophers have taken ſuch an enlarged view of this power of heat, that they have not only ſuppoſed the moſt fixed bodies might be volatilized, had we a ſufficient heat in our power, but that every

palpable, elaſtic, fluid in nature, is produced and preſerved in that form by the action of heat. Mr. Amontons, an ingenious member of the late Royal Academy of Sciences at Paris, was the firſt who propoſed this idea with reſpect to the atmoſphere. He ſuppoſed that it might be deprived of the whole of its elaſticity, and condenſed, and even frozen into a ſolid matter, were it in our power to apply to it a ſufficient cold; that it is a ſubſtance which differs from others, by being incomparably more volatile; and which is therefore converted into vapour, and preſerved in that form, by a weaker heat than any that ever happens or can obtain in this globe, and which therefore cannot appear under any other form than the one it now wears, ſo long as the conſtitution of the world remains the ſame as at preſent.

This opinion, though it may appear, at the firſt view of it, to be an extravagant flight of the imagination, is, however, ſupported both by analogy, and, in ſome meaſure, by direct experience. We know that water is eaſily convertible by heat into a vapour, which, ſo long as it is kept ſufficiently hot, has many of the qualities of air. It is very light and rare, even more ſo than air, and it is equally tranſparent. It is alſo very compreſſible, and reacts with an high degree of elaſticity againſt the force compreſſing it, in the ſame manner as air; and its elaſticity is greatly increaſed by additions of heat, as well as diminiſhed by the abſtraction of it.

As heat can thus produce from water an elaſtic vapour, which reſembles air in ſo many of its qualities, it is not improbable that air itſelf is produced in the ſame manner from ſome other matter. The preſumption is very ſtrong; and, when we conſider the effects of heat on air more attentively, we find reaſon to be the more perſuaded that its rarity and elaſticity depend upon heat; for the more its heat is increaſed, it becomes always the more elaſtic, and, if it has room to expand, the more rare: And although we are not able to cool it ſo much as by that way alone to reduce it to a denſe unelaſtic body, we find, however, that the more it is cooled, its elaſticity is the more diminiſh-

ed; and it is the more condenſed, without any bounds to this condenſation and diminution of elaſticity, except our inability to make it colder.

In the modern doctrines of chemiſtry, therefore, it is now conſidered as an indubitable truth, that the air has its rare and elaſtic form from the heat it contains; and it is aſſumed as an eſtabliſhed principle, that all other permanently elaſtic fluids derive their elaſtic vaporous form from the ſame cauſe; that each of them is a compound of a peculiar matter, and of the matter of heat, united in the ſtate of latent heat, (or caloric, as it is named by the French chemiſts,) with the other matter, ſo as to conſtitute with it an elaſtic fluid or vapour. I obſerve, that Fourcroy, in his *Elemens de Chymie*, ſays expreſsly, that by CALORIQUE, his countrymen mean the ſame thing that I named latent heat, and that they reſerved the common word CHALEUR to expreſs moveable or ſenſible heat. I am not altogether certain of Fourcroy's being ſufficiently warranted in this uſe of the words, nor do I think it the moſt proper. Were I to write French, I would uſe the term *chaleur latente* for thoſe combinations from which heat is abſtracted by the touch of a colder body, and I would reſtrict the term *calorique* to the combinations in which it is indeed equally concealed, but from which it cannot be extracted, but by preſenting a *proper* third body, at the ſame, or even at a higher temperature, namely, to the gaſeous combinations, whence it is generally diſengaged, accompanied with light, exhibiting the phenomenon called combuſtion. I am the more diſpoſed to make this diſtinction, becauſe I think that this is a general mode of combination, containing ſeveral different ſteps, analogous to thoſe obſerved in the combinations of latent heat. But of this we ſhall occaſionally hear more afterwards.

OF EVAPORATION.

Thus, we have come near the end of this ſubject; but to make it complete, it is neceſſary to obſerve, that, beſides the elaſtic ſteam pro-

duced from volatile bodies, when a ſufficient quantity of heat is thrown into them, there is a different kind of vapour produced from many of them, in much lower degrees of heat than what is neceſſary to the production of the former. But this ſecond ſort of vapour is always produced ſlowly and imperceptibly, without the ſmalleſt commotion or agitation, and only occaſions a very ſlow and gradual diminution of the maſs, until it be entirely changed, and diſappear. Thus, camphor, or volatile ſalts, even though wrapped in paper, or other ſuch containing matters, are continually emitting vapours, which are perceptible to the ſmell, though inviſible to the eye, and are thus gradually waſted, until they are entirely loſt.

We have a familiar inſtance of this ſort of vaporiſation, as well as of the other, in water. If water be expoſed in open veſſels, it is ſure to exhale away imperceptibly, in this manner, until it totally diſappears, whatever be the temperature of the air to which it is expoſed; faſter, indeed, as the weather is warmer; but it has been obſerved, that even in froſt, and when the water itſelf is in the form of ice, it always ſuffers ſome loſs this way.

This, to diſtinguiſh it from the former, may be called NATURAL EVAPORATION. It happens through a conſiderable latitude of heat, but becomes more remarkable, as the heat is increaſed. The vapour thus produced, differs, by an obvious diſtinction, from the one I have hitherto deſcribed, and which may be called the *elaſtic vaporiſation*, which is produced only by increaſing the heat to the boiling degree. The vapour, produced by the boiling heat, and which may be called ſteam, has great elaſticity and force, and requires violence to confine it; whereas that produced in natural evaporation, unleſs it be made as hot as the other, has no elaſticity that is perceptible in ordinary circumſtances, but may be detained in the weakeſt veſſels, with eaſe, and does not exert the ſmalleſt ſenſible force upon the veſſel which confines it. Every body knows that water can be preſerved for any length of time in cloſe veſſels of glaſs, however thin. Provided

they be ſuch as that the water cannot penetrate or diſſolve them, it never will be diminiſhed in quantity, nor will it produce any vapour that will ſhew the ſmalleſt diſpoſition to burſt them. As this ſort of evaporation of water expoſed to the air goes on always in a quiet and imperceptible manner, without any commotion or agitation of the fluid, it is plain that it muſt proceed entirely from the ſurface: And accordingly, it appears by experiments that it is always more copious, as the ſurface is greater.

Another obſervation, with regard to this kind of evaporation, muſt not be omitted; that if this ſort of vapour be allowed to ſtagnate over the body, or be confined upon its ſurface, it proves an obſtacle to the formation of any more, or to the progreſs of evaporation. In order to have a continued production of this vapour, it is neceſſary that it be carried off, or removed from the ſurface as faſt as it is formed.

Hence wind greatly promotes natural evaporation. I likewiſe obſerved before, that it is greatly promoted by heat. The production of this vapour does, indeed, ſo much depend upon heat, that when, in conſequence of more heat than ordinary applied to bodies, a great quantity has been produced, it is condenſed again, like the other, when this heat is diminiſhed. Thus, when a bottle or glaſs of cold water is brought into the room where many perſons have been ſitting for ſome time, its outer ſurface is immediately dimmed by the vapour contained in the air of the room, now condenſed upon it. Hence, too, ariſe the moiſture and hoar-froſts, on the inſide of our windows, in very cold weather; as alſo on our walls, when a hard froſt is followed by a ſudden thaw, brought on by a wind coming from the ſouth, and filled with vapour. It is a vulgar error to aſcribe this moiſture on our ſtone-walls to the weather penetrating through them. It is the vapour contained within the houſe, condenſed by the cold walls. We obſerve none when the walls are covered with laths and plaſter, and think that this prevents the penetration. But the true reaſon is, that this thin, detached covering, ſoon acquires the warmth

of the houſe, and condenſes no more vapour. The glaſs of our windows condenſes it copiouſly, becauſe very cold, yet it is impenetrable by the weather.

From the ſame cauſe ariſe the miſts, which hover over low marſhy grounds in the cold mornings and evenings, eſpecially after a warm day, in which the evaporation has been very copious. In this caſe, the ſmalleſt diminution of heat produces a condenſation, and the vapour becomes viſible, and is precipitated. The miſt ſeems to riſe, and we ſee a ſtratum of clear air below the ſtratum of dew, becauſe the precipitation begins in what is next the ground, and proceeds gradually upwards, as the ſtrata cool in ſucceſſion. For the ſame reaſon, we frequently ſee a ſmoke apparently riſing from holes made in the ice which covers deep lakes and rivers. *(See Note 6. at the end of the Volume.)*

This ſort of vaporiſation, and particularly the natural evaporation of water, has been much attended to, as being cloſely connected with ſome of the more important and extenſive operations of nature.

By this ſort of vaporiſation, water ariſes into the atmoſphere, from the ſurface of the ſea, and other large collections, in the form of tranſparent vapour, which, at a certain height, is condenſed by the cold of the ſuperior regions, into miſt or clouds. Theſe clouds are ſuſpended at different heights, probably in conſequence of the particular temperature and rarefaction of that portion of air which is contained in them, and they are liable to become electrified, ſo as ſometimes to occaſion lightning and thunder. Often alſo, in conſequence of a more quick and perfect condenſation of the vapour, or by means of an electrical attraction of the little globules of water for one another, they form drops of water, which deſcend in rain, which is indiſpenſably requiſite for the nouriſhment of vegetables and animals, and gives origin to ſprings, rivers, and lakes.

As this circulation of water, by which we all live, depends entirely on natural vaporiſation, it has greatly attracted the attention of phi-

loſophers, and has occaſioned many attempts to diſcover and explain in what manner it happens.

1. The firſt remarkable attempt was made by Dr. Derham, in his *Phyſico-Theology*, B. I. ch. 2. and B. II. ch. 5. He ſays that fire, being the lighteſt of all natural things, riſes up through the water, and carries off with it a quantity of water. The fire, burſting out of the water, is enveloped with a thin film, in the form of a ſmall bladder, or veſicula, which riſes in the air by its extreme lightneſs, till it attains a height correſponding to its ſpecific gravity. Myriads of ſuch veſiculæ form clouds, which float about in the atmoſphere, till they are condenſed by cold, or coaleſce by being driven againſt each other by the winds. Then they deſcend in dews, fogs, rains, ſnow, or hail.

This was a very careleſs attempt to explain ſo curious and important a phenomenon. Veſicles of ſuch a conſtitution could not remain a minute in the air, but muſt collapſe as ſoon as they are formed. Nay, we cannot conceive their formation. Fire has never been met with in the form of a palpable expanſive fluid, like air, fit for diſtending theſe veſicles. They muſt inſtantly collapſe, acquiring the temperature of the air; and now, being much heavier, muſt immediately fall down again.

2. The theory of Deſaguiliers is founded on calculations of the ſpace which he ſuppoſed water to occupy, when changed into vapour, with different degrees of heat. He firſt endeavours to calculate how much, or to how many times its own bulk, water is expanded, when changed into ſteam by the boiling heat, or elaſtic vaporiſation, under the preſſure of the atmoſphere; and, from ſome premiſes, he concludes that, when changed into ſteam, under that preſſure, it is expanded to 14000 times the bulk of the water. In the next place, he ſuppoſes that the different degrees of heat, between froſt and the ordinary boiling point of water, muſt produce from water vapours leſs rarefied, but ſtill very rare, and approaching in rarity to the ſteam of boiling water, in proportion as the heats by which they are produced approach to that heat, and are diſtant from froſt, or the cold of melting ſnow. And,

from theſe principles, he concludes, that the heat of the weather in ſummer, ſpring, and autumn, and even in the greater part of winter, is ſtill ſufficient to form vapours from water, which ſhall be lighter than air; and therefore diſpoſed to aſcend, or to be driven upwards in the atmoſphere.

But his very firſt propoſition is totally falſe, and the falſity of it overthrows the whole of his reaſoning. Water, when changed into ſteam, is not expanded into 14000 times its ordinary bulk. I examined, with great attention, that part of his book in which he has ſtated this propoſition, to ſee if I could learn on what it was founded. I could not obtain the leaſt ſatisfaction: And I can aſſure you, that, by a ſimple and deciſive experiment, which I lately deſcribed to you, the ſpace which water occupies, when changed into ſteam, under the ordinary preſſure of the atmoſphere, is only about 1664 times its bulk, when in the form of water. More recent experiments by Mr. Watt, make 1720, or rather more; but he is not perfectly preciſe in his opinion.

3. A much more ingenious explanation was propoſed by the celebrated aſtronomer and philoſopher Dr. Halley. In ſome of his writings, indeed, he gives Derham's explanation of the riſe of vapours by veſicles, diſtended, not by fire, but by heated air. (See Phil. Tranſ. No. 192.) This is as untenable as the other. But Dr. Halley, afterwards, in a diſſertation on the origin of ſprings and rivers (See Phil. Tranſ. abridged, vol. 2. No. 127.), expreſſes himſelf as follows:

"I take it that the air itſelf will imbibe a certain quantity of aque-
"ous vapours, and retain them, like ſalts diſſolved in water. The
"ſun warming the air, and raiſing a more plentiful vapour from the
"water in the day-time, the air will ſuſtain a greater portion of va-
"pour, juſt as warm water will hold more ſalts diſſolved, which, upon
"the abſence of the ſun in the nights, will all be again diſcharged in
"dews, analogous to the precipitation of ſalts on the cooling of the
"liquors."

It is ſurpriſing that an explanation, ſuggeſted by ſo eminent a philoſopher, and ſupported by ſo extenſive analogy, did not meet with more general acceptance. Although this eſſay of Dr. Halley's, and another much connected with it, are frequently quoted with the greateſt reſpect, eſpecially on the Continent, this paſſage is never mentioned. And in 1743, when the Academy of Sciences of Paris publiſhed this as a prize queſtion, the many performances of the competitors, and even thoſe of Kratzenſtein and Hamberger, which ſhared the prize, take no notice of it. But the explanation given by theſe authors are very inadequate indeed; yet the theory of Kratzenſtein, who adopts the veſicular form of the matter which compoſes cloud, fog, or dew, is accompanied by many very curious obſervations and ingenious reflections on theſe veſicles. He produces the veſicles, by forcing a quantity of air into a clear glaſs globe from the lungs; the globe has a leather valve, which keeps in the air; when this is pierced with a pin, the air comes out ſlowly, and the veſicles appear in the glaſs in ſtrata, exhibiting beautiful colours. In 1750, however, the ſame Hamberger, in his *Elementa Phyſices*, publiſhed at Halle, adopts Dr. Halley's ſolution-ſyſtem completely, but without ſeeming to have taken it from him, ſo unſkilfully does he manage it.

In the following year, Mr. Le Roy, phyſician at Montpellier, (I think) and correſpondent of the Academy of Sciences, gives this ſyſtem in full detail, ſupported by a numerous train of reſemblances to the ſolution of ſubſtances in their ſolvents, and applied to the chief phenomena with great diſtinctneſs and ingenuity. The celebrated Dr. Franklin, the late Lord Kaims, and Dr. Hamilton of Trinity College Dublin, have alſo publiſhed diſſertations to the ſame purpoſe, ſupported by the ſame arguments, and applied nearly in the ſame manner.

The circumſtances adduced by theſe authors, in which the riſe of vapour, and the principal phenomena of the atmoſphere, reſemble chemical ſolutions, are indeed very many and very ſpecious.

There is an actual ſolution, for the dampeſt air is perfectly tranſparent: bodies dry very faſt in warm air, but ſlowly in cold, and in the cool of evening, the dews are more copious after a warm day. Every temperature of the air appears to have an appropriate power of taking up vapour, and part is precipitated as it cools. This perfectly reſembles the ſaturation which obtains in all ſolutions. Extent of ſurface greatly promotes evaporation. More water will evaporate from a moiſt cloth in an hour, than from a bowl of water in a day. Wind alſo greatly promotes evaporation, juſt as a renewal of water or agitation promotes ſolution. Stagnation, on the contrary, puts a ſtop to both.

To theſe arguments may be added, that evaporation produces cold, in proportion to its extent and rapidity, juſt as it is produced in the ſolution of ſalt.

This theory of the aſcent of vapours, when thus propoſed, ſoon obtained a general concurrence, and was freely employed by the meteorologiſts. But an experiment related by Wallerius, in which he obſerved a copious evaporation in the exhauſted receiver of an air-pump, raiſed ſome doubts about the validity of this explanation. The experiment has been repeated with great care by many philoſophers, and it is now well known, that evaporation goes on much faſter *in vacuo* than in the open air, and is more and more retarded, in ſimilar circumſtances of temperature, &c. in proportion as the evaporating body is ſurrounded by denſer air. It is found that heat expands water into elaſtic vapour in all temperatures. In an experiment by Mr. Watt, water of the temperature 57° produced a vapour which ſupported half an inch of mercury. This experiment was a very curious one. A ſingle drop of water was let into a very fine barometer; as ſoon as it got above the mercury in the tube, the mercury ſunk half an inch. Mercury was now poured into the ciſtern, and it roſe alſo in the tube; but the mercury in the tube ſtill wanted half an inch of its firſt elevation above that in the ciſtern. This depreſſion continued while mer-

cury was ſtill poured into the ciſtern, till at laſt the vapour of the water was all condenſed again. This is analogous to an experiment which I mentioned ſome time ago, in which boiling hot ſteam was condenſed by preſſure.

Now, in this experiment of Mr. Watt's, there was no air to diſſolve the water. Mr. Nairne produced, in like manner, vapour of the ſame ſtrength in the air-pump receiver, from a bit of moiſt paper. Mr. Sauſſure ſet a barometer into a tall glaſs, the air of which was rendered perfectly dry by means of quick-lime, which abſorbed all moiſture. He then threw into it a ſmall piece of moiſt linen, and inſtantly ſhut the apparatus. In a ſhort time, the vapour produced from the cloth raiſed the barometer half an inch. Comparing theſe three experiments, we ſee that the preſence of the air, in the laſt experiment, had not contributed to the production of vapour.

It is therefore conſidered as very doubtful, at leaſt, whether the active diſſolving power of the air is the cauſe of the riſe of vapour in the atmoſphere; and it ſeems to be the moſt prevailing opinion that heat is the ſole agent. Thoſe who continue to maintain Le Roy's ſyſtem, (Mr. de la Place and Mr. Monge) ſay that there is leſs cold produced in ſpontaneous evaporation in the air, than in the artificial evaporation *in vacuo:* But another experiment by Mr. Watt ſhews the contrary. Two veſſels, perfectly ſimilar, were expoſed, filled with water of the ſame temperature; the evaporation from one of them was prevented by a covering of oil; the heats loſt by both were compared, and the grains of water evaporated were meaſured; the difference of the heats loſt greatly exceeded the heat loſt by evaporating the ſame quantity of water in the air with a boiling heat. Thus, the ſolution-ſyſtem ſeems to loſe ground. Mr. Sauſſure thinks that heat alone expands the water into vapour, and that the incumbent air diſſolves this vapour. Mr. De Luc has another theory, but it is too complicated to be conſidered here. See his *Meteorologie.* *(See Note 7. at the end of the Volume.)*

I mentioned the cold produced by ſpontaneous evaporation. Dr. Cullen's experiments, which I adduced on another occaſion, are the moſt remarkable examples. Mr. Mairan and Mr. Franklin give many examples of the cold produced by the evaporation of water and other fluids from the ſkin. It is from this cauſe that a wet cloth, expoſed to the air, often freezes ſtiff. Although the thermometer ſtands ſome degrees above 32°, water evaporates, and this cools the remainder ſo as to freeze it.

When theſe phenomena were firſt taken notice of by our philoſophers, they were thought very wonderful; but they have been long familiarly known in India and in China. In thoſe ſultry regions, water is kept cool by putting it into very porous earthen jars. Theſe are ever covered with moiſture, in a ſtate of copious evaporation. The traveller carries water in an earthen mug of the ſame kind, or covers his can with woollen cloth; he has this ſlung on his ſhoulders, and dips it into every pool or puddle. The land-winds being exceſſively dry, cauſe the water to evaporate very faſt, and a great deal of heat is thus abſorbed. The wealthy have their dining halls open on all ſides, the roof being ſupported on pillars, and the intervals are hung with curtains. Servants without doors ſcatter water continually on theſe curtains, and its evaporation abſorbs a vaſt deal of heat, or makes the hall cool and refreſhing.

Nay, ſuch is the effect of evaporation in abſorbing heat, that the rich inhabitants of thoſe countries can lay up ſtores of artificial ice, in a climate where the thermometer is never under 40°, and frequently riſes to 110°. I ſhall explain the curious proceſs for this purpoſe, as I have it from a gentleman who reſided nine years at Benares, one of the ancient ſeats of arts and ſciences. He ſaw the proceſs performed ſeveral times, and his letter on the ſubject is to be ſeen in the Philoſophical Tranſactions for 1792 or 1793.

Benares is between the 25th and 26th degree of north latitude, a great way up the Ganges, and has an immenſe continent extending

from it in every direction. The proceſs is performed in the middle of their winter only, and in clear dry weather, when the thermometer falls to within 8 degrees of the freezing point; and although no ice is ever formed on the pools and other ſmall collections of water, they ſucceed in procuring it by this proceſs.

Shallow pits or beds are made four or five feet wide, and about four inches deep, ſeparated from one another by narrow borders, and ſo numerous as to cover an extent of about four acres. Theſe pits are filled with dry ſtraw in the proper ſeaſon, and on the ſtraw are placed rows, cloſe together, of ſhallow earthen pans, into which a few inches depth of water are put every evening of the winter ſeaſon, when the weather is clear, and the nights very cool. In the morning they find more or leſs ice in the pans, which is taken out before ſun-riſe, or about five o'clock; and, that it may ſeparate the more eaſily from the earthen veſſels, the inſide of theſe is rubbed with a little butter before the water is put into them. The number of earthen veſſels or pans in one place is about 100,000; and about 300 men, women, and children, are employed in the morning in gathering the ice, that it may be done quickly, before the heat of the day begins. They wrap it in thick coarſe woollen ſtuff, and carry it to the ice-houſe. The clear and cold weather, which is neceſſary to the production of the ice, by this proceſs, is undoubtedly attended with a northerly wind. And in ſuch a country, the northerly winds, or breezes, are exceſſively dry, in conſequence of their coming over a great extent of dry northern continent. They have, therefore, great power to promote evaporation. The ſhallow earthen veſſels, expoſed in this manner, and the water in them, muſt become colder, by the quicker than ordinary evaporation of a part of the water, than any other matter around them. Care alſo is taken, by the contrivance of the proceſs, to prevent the coldneſs of the veſſel and water from being too much diminiſhed by communication with the warm ſoil that is below. The

thick bed of ſtraw, on which the veſſels are laid, is evidently intended to ſerve this purpoſe.

In another part of India, farther ſouth, or nearer to Calcutta, they find more difficulty in preparing ice by a proceſs of this kind. But experience has taught them, that boiling the water, before it is expoſed, promotes their ſucceſs. This fact was communicated to me in the year 1774, or 1775, by the late Sir John Pringle, Preſident of the Royal Society, who deſired to have my opinion on it. Before I gave any opinion, I thought it was proper to inquire into the certainty of the fact, and I accordingly made ſome experiments relating to it. But I cannot ſay that they terminated in any very confident opinion. I found very little difference, if any, in the quantity of ice produced from boiled and unboiled water. I think that, in moſt of my trials, the boiled water firſt began to freeze, and, when the heat of the air was but very little below 32°, ſuppoſe not lower than 29°, the difference was certain, and more conſiderable. I found always, that when the one had frozen a little, and the other not, the ſmalleſt touch or diſturbance, with a tooth-pick, made it freeze inſtantly. The difference, therefore, which I think may be ſtated between them, is that boiled water begins to freeze as ſoon as the air has cooled it to 32°, but that unboiled water will continue fluid a little longer, if not diſturbed. I recollect that the experiment in which this curious fact was firſt obſerved, was made by Fahrenheit, with water which had been purged of air by boiling. He had ſealed the veſſel containing it. Wiſhing to apply a thermometer, he broke off the end of the neck, and the water froze as ſoon as the air touched it. Perhaps this was effected by the water's beginning immediately to abſorb air again. Perhaps air, as well as ſalt, retards the congelation of water.

On the whole, this operation is an example of a curious art, which, in its preſent ſtate of perfection and ſimplicity, ſeems the product of great knowledge and ingenuity, requiring uncommon talents in any individual to invent and direct it. But it probably even began in an-

cient times, in a cooler climate, and extended ſlowly ſouthward, in conſequence of repeated improvements, derived from experience, the ſource from which all our uſeful knowledge has proceeded.

I am perſuaded that the heat abſorbed in ſpontaneous evaporation greatly contributes to enable animals to bear the heat of the tropical climates, where the thermometer frequently continues to ſhew the temperature of the human body. Such heats, indeed, are barely ſupportable, and enervate the animal, making it lazy and indolent, indulging in the moſt relaxed poſtures, and avoiding every exertion of body or mind. The inhabitants are induced to drink large draughts of diluting liquors, which tranſude through their pores moſt copiouſly, carrying off with them a vaſt deal of this troubleſome and exhauſting heat. Still, however, theſe heats are moſt oppreſſive to the poor, who muſt work, and often produce great mortality, when they continue long unabated. There is in the body itſelf a continual laboratory, or manufacture of heat, and, were the ſurrounding air of ſuch a temperature as not to carry it off, it would ſoon accumulate ſo as to deſtroy life. The exceſſive perſpiration, ſupplied by diluting draughts, performs the ſame office as the cold air without the tropics, in guarding us from this fatal accumulation. It is not unlikely that that conſtitution of the veſſels of the lungs, and the pores of the ſkin, which unfits them from bringing forward the lymph, in ſufficient quantity for carrying off, by evaporation, the heat generated by the vital functions, is the immediate cauſe of the heat, in ordinary fevers.

It is really wonderful what heats may be carried off in this way. Dr. Fordyce ſtaid for a conſiderable time, and without great inconvenience, in a room heated to 260° of Fahrenheit's ſcale. The lock of the door, his watch and keys lying on the table, could not be touched without burning his hand. An egg became hard; his pulſe was at 139°; yet a thermometer held in his mouth was only 2° or 3° hotter than ordinary. He perſpired moſt profuſely. (See many

curious and inſtructive obſervations and reflections on theſe experiments in Phil. Tranſ. vol. 64. and 65.)

There is only one phenomenon remaining that may be thought worthy of notice, and it was diſcovered by the worthy and ingenious Profeſſor of Aſtronomy at Glaſgow, Mr. Wilſon, and deſcribed by him in a paper of the firſt volume of the Tranſactions of the Royal Society of Edinburgh; and he alſo communicated ſome of his obſervations on this ſubject to the Royal Society at London.

During a winter, when the cold was uncommonly ſevere and intenſe in this country, and when the thermometer fell ſome nights to the beginning of Fahrenheit's ſcale, and, even in the day time, ſtood for many hours at 10 or 12 degrees only above it, the Profeſſor, who paſſed ſome part of the night in the Obſervatory, took with him ſeveral thermometers, and placed them in different ſituations. Some were laid on the ſurface of the ſnow, others hung up in the air, and otherwiſe; and he very ſoon perceived, that thoſe which were laid on the ſurface of the ſnow fell conſiderably lower than the reſt; ſometimes they were 14 degrees lower. He then ſunk ſome of them below the ſurface of the ſnow, which was uncommonly deep, and found, as he expected, that they roſe and ſtood higher than when they were hung up in the air; they roſe the higher, the more deeply they were ſunk in the ſnow, and the nearer they were placed to the ſurface of the ground. This was eaſily underſtood, but the ſurpriſing and unaccountable fact was, that they ſhould indicate a greater degree of cold when laid on the upper ſurface of the ſnow, than when hung up in the air above it. This fact excited his curioſity, and he made a great number of judicious experiments to diſcover the circumſtances with which it was connected. He learned by theſe experiments, that it was connected with a depoſition of hoar-froſt, and that it was only when the thermometer was placed on a ſurface of ſnow or other matter, on which a depoſition or condenſation of hoar-froſt was going on, that it ſunk ſo low. This he aſcertained by a number of deciſive

experiments: But he obſerved that it happened only when the cold of the air was at the ſame time violent, ſo as to make the thermometer fall 20 degrees, or more, below froſt. Still, however, it was a moſt unexpected and ſurpriſing phenomenon. From the knowledge we have of latent heat, we might rather expect to ſee an *increaſe* of *ſenſible heat*, on this occaſion, than a *diminution* of it. *(See Note 8. at the end of the Volume.)*

SECT. IV.—OF IGNITION.

WE are now to attend to the fourth general effect of heat, commonly called IGNITION, or INCANDESCENCE.

By this term, is meant that glow, or emiſſion of light from bodies, which appears when their heat is increaſed to an high degree of intenſity, about the 650° of Fahrenheit's ſcale; and which, when it firſt begins, is weak and obſcure, and of a red colour, but increaſes in brightneſs as the heat becomes ſtronger, until, in the moſt violent intenſity of heat which we can produce, it becomes ſo bright and dazzling a whiteneſs, that the eye cannot bear it.

A difficult queſtion here occurs, concerning the connection between heat and light: How happens it, that each is naturally diſpoſed to produce the other? We know, by experience, that whenever a body is heated intenſely, it always becomes luminous, or emits light; and, on the other hand, it appears as plain, from the effect of the light of the ſun, the moſt luminous body in nature, and, eſpecially, when this light is condenſed by convex glaſſes, or concave mirrors, that a ſtrong light, directed upon bodies, is ſure to produce in them very quickly the moſt violent heat.

The opinion of ſome of thoſe who have ventured to form conjectures on this ſubject, is, that light, as well as heat, is produced by that ſubtile, elaſtic, ſelf-repellent matter, which has been imagined to be the cauſe of the ſenſation heat: That when the matter, in its ordi-

nary and moſt communicable ſtate, abounds in a body, the body appears hot, or is in a hot ſtate; but that, when it abounds to a high degree, ſome particles of this ſubtile matter are thrown off with inconceivable velocity, correſponding to its high degree of elaſticity, or the powerful repulſion of its parts for one another; and that theſe projected atoms conſtitute light. *(See Note 9. at the end of the Volume.)*

Dr. Boerhaave conſiders the connection between heat and light at great length in his treatiſe on fire; but he perplexes himſelf, and gets into a confuſion of ideas that is ſurpriſing. He takes much pains to find the reaſon,—why the light of the moon, though collected by mirrors or burning glaſſes, ſhould not ſenſibly affect the moſt delicate thermometer; while that of the ſun, collected by the ſame means, produces very quickly the moſt vehement heat of which we have any experience, and has great effect on bodies even when it is not condenſed. At laſt, he thinks he has found the reaſon of the difference, which is, that the rays of the ſun are more parallel than thoſe of the moon.

But this is the moſt unfounded notion that can be imagined. Neither the rays that come from the ſun, nor thoſe that come from the moon, are parallel to one another. Thoſe, indeed, that come from any ſingle point of either of theſe luminaries, are very nearly parallel when they reach us. On account of their coming from a point at ſuch a very great diſtance, they are ſo nearly parallel, that they appear, by experiments, to be exactly ſo; and there is no ſenſible difference between the rays of the ſun, and thoſe of the moon, in this reſpect. Though the ſun is more diſtant than the moon, the moon is at too great diſtance to admit of our perceiving the ſmalleſt ſenſible divergency in the rays that come from the ſame point of her ſurface. But if we conſider the rays which come from different parts of the ſurface, either of the ſun or of the moon, it is plain that they are far from being parallel. The ſun's diſk ſubtends an angle of half a degree; and his rays, if we conſider the whole of them together, are not ſo paral-

lel as thoſe of the moon, whoſe diameter ſubtends an angle not quite ſo large. It is, therefore, ſurpriſing that Dr. Boerhaave ſhould have taken up this idea, which, at any rate, does not in the leaſt make the fact more intelligible. If the effect of the ſun's rays, in heating the bodies upon which they fall, depend upon their paralleliſm, the light which comes from Jupiter and Saturn, in ſome of their poſitions, ſhould be more powerful than that of the ſun.

The true cauſe of the great difference between the effects of the rays of the ſun and thoſe of the moon is the exceſſive feebleneſs of the moon's light.

The moon is plainly illuminated by the ſun, and the light which comes from her to us, is a part of the ſun's light reflected from her ſurface. But it is plain that, even ſuppoſing it to be all reflected, (which is very improbable) this reflection muſt happen in ſuch a manner, that the light is diſperſed equally in all directions; and, therefore, a very ſmall part of it only can come toward the earth; ſuch a ſmall part, that it is rather difficult to underſtand why ſhe appears ſo bright. Accordingly, ſome philoſophers think that the ſurface of the moon muſt be of the very brighteſt kind, as if it were covered with ſnow. But I do not think it neceſſary to form ſuch a ſuppoſition, in order to account for her apparent brightneſs at night. I was ſurpriſed at the appearance which a baloon of oiled paper, filled with inflammable air, made in a ſerene blue ſky, and bright ſunſhine, though the reflection of light from it, when near the earth, was not remarkable. It appeared like a ſtar, or like Venus, when it was ſo high that its ſhape was not ſeen.

That the light reflected by the moon to our earth is, in reality, next to nothing, when compared with that of the ſun, is plain from ſome obvious and eaſy calculations.

Bouguer's experiments were made by cauſing a beam of the ſun's light to diverge by means of a concave glaſs. Thus the light was weakened as it receded from the glaſs, the ſtrength of it being, at dif-

ferent diſtances from the focus of glaſs, inverſely as the ſquares of theſe diſtances. Then he inveſtigated, by experiment, the diſtance at which it was equal in ſtrength to the light of a candle thrown upon paper, placed at proper diſtances from the candle. And he afterwards compared the light given by the ſame candle with that of the moon. In making this laſt compariſon, it was not neceſſary to weaken the light of the moon, in order to make it equal to that of the candle. It was, on the contrary, neceſſary to weaken that of the candle. The final reſult of Mr. Bouguer's calculations is, that the ſun's light is more than 300,000 times denſer than that of the moon; which laſt is, therefore, quite undiſcoverable by our moſt delicate thermometers.

Ignition may be conſidered as one of thoſe effects of heat that is produced in the moſt ſimilar manner, upon different bodies, of any kind whatever. It is alſo a very general effect of heat. We do not know of any bodies that are exempted from it, unleſs it be thoſe whoſe volatility is ſo great, that they cannot be made to endure the degree of heat neceſſary for this effect. But even theſe, when in the form of vapour, may perhaps be heated red hot, though many of them muſt be ſo very greatly expanded before it acquire the neceſſary degree, that it would not be eaſy to perceive whether or not they are in the ſtate of ignition. *(See Note* 10. *at the end of the Volume.)*

Upon the whole, we have reaſon to conſider this as an effect of heat, to which all bodies, without exception, are liable, and all at the ſame, or nearly the ſame, degree of heat. But it is difficult to know exactly the place of this degree in the ſcale of heat. We depend on the acuteneſs of our ſight to inform us of the beginning of ignition, and we are not ſure that it perceives the very weakeſt light. The preſumption is rather the contrary, as we know that other animals ſee objects in ſuch weak light, as appears to us to be utter darkneſs. And even our eyes will, in one condition, be affected by a weak light, to which they are quite inſenſible in another.

Sir Iſaac Newton, however, attempted to eſtimate the degree of heat, which is attended with the weakeſt perceptible light, by the method I formerly deſcribed, viz. expoſing a lump of very hot iron to a ſtream of cold air, and obſerving exactly the time when it ceaſed to emit light; and having alſo obſerved the celerity and progreſſion with which it cooled, or loſt its heat, from the beginning of the experiment until it was reduced to the temperature of the air, he was enabled to eſtimate the heat which remained in it at the laſt moment of its ignition.

By a calculation of this kind, he reckons that iron, in the loweſt ſtate of ignition, or when it juſt ceaſes to ſhine perceptibly in the dark, is heated to the 635th degree, in a prolongation of Fahrenheit's ſcale; that it juſt ceaſed to ſhine in the twilight, when it was heated to the 884th degree; and that, when it was as red hot as a common fire could make it, the degree of its heat was 1049 or 1050.

To conceive, with ſome diſtinctneſs, the relations which theſe intenſe heats have to the temperatures with which we are more familiarly acquainted, it is by no means enough to attend to the numbers which expreſs them. This gives but a very imperfect notion of the comparative magnitude of their intervals. We muſt have recourſe to a ſcale, or ſtraight line, divided into a great number of equal parts, or units, and place the phyſical character, or effect of each temperature, at the correſponding units of this ſcale. By looking at ſuch a figure, we perceive at once, that the difference between the heats of boiling oil of turpentine, and boiling water, is nearly twice as great as that between the heats of boiling and of freezing water; and that this laſt difference is nearly the ſame with that of boiling mercury and melting tin, &c. The inſpection of ſuch a ſcale ſhews us how very ſmall a part of the ſcale of meaſurable heats comprehends all the temperatures of the habitable climates, and thoſe of our moſt familiar and intereſting experiments.

But the temperature of glowing iron is far from being the higheſt of thoſe of many chemical operations, with which it is neceſſary to

have a ſort of practical acquaintance. Some metals are obſerved, by thoſe who manufacture them, to melt when red hot, ſome before, and ſome after they are red hot. The cutlers have familiar marks when ſteel and iron are fit for certain operations, and have terms which expreſs thoſe ſtates, ſuch as, *cherry red*, *worm red*, a *white heat*, a *blue white*, &c.

You will readily perceive that many of the temperatures in the upper part of the ſcale are ſuch as cannot be determined by the expanſion of a thermometer. They have been determined in Newton's way, (indeed by Newton himſelf) already deſcribed. Some of them have been determined by Mr. Irvin, at my deſire, by means of the heat communicated to water, by the lump of iron, immediately after the freezing of the metal. Some of Newton's determinations were by his lintſeed oil thermometer. With reſpect to the temperatures far below froſt, I have paid no regard to thoſe low ſtations of the mercurial thermometer deſcribed by Profeſſor Braun. Theſe were undoubtedly owing to the irregular contractions of portions of the mercury, as they froze. But as the ſpirit thermometer pointed at the ſame time to —148°, I have conſidered this as the temperature of the experiment. For the ſame reaſon, I have admitted —120° as a natural cold, once obſerved in Siberia.

TABLE OF SENSIBLE HEATS.

1050 Iron, as hot as a common fire could make it.
884 Iron red, in the twilight.
752 Iron bright red, in the dark.
635 Loweſt ignition of iron, in the dark.
600 Mercury, and alſo ol. lini, boil (644 according to Mr. De Secondat.)
560 Ol. Terebinth boils.
546 Sulphuric acid boils.
540 Lead melts (Newton) 585 (Secondat) 594 (Irvin).
460 Biſmuth melts. 476 (Irvin.) alſo tin 1 pt. and lead 4 pts.
408 Tin melts. 413 Irvin.
334 Tin, 3 pts. lead, 2 pts. melt. Alſo tin, 2 pts. biſm. 1 pt.
283 Tin, 1 pt. biſm. 1 pt.
242 Nitric acid boils.
212 Water boils. Biſm. 5 pts. tin 3 pts. and lead 2 pts. melt.

174 Alcohol boils.
156 Serum and Albumen ovi coagulate.
142 Bees-wax melts, Irvin.
107 Feveriſh heat.
100 to 96, Animal heat, or blood-warm.
64 Summer-heat in this climate.
32 Froſt.
30 Milk freezes.
28 Vinegar freezes.
25 Blood (human) freezes.
20 Strong wines freeze.
7 Spirit of wine 1 pt. mixed with 3 pts. water, freezes.
0 to $\frac{0}{4}$, or from 0 to —4, ſalt and ſnow melt.
—7 Brandy, or equal pts. of alcohol and water, freezes.
—11 Spirit of wine, 2 pts. mixed with 1 pt. water, (Reaumur).
—14 Cold, obſerved in the air at Glaſgow Obſervatory, anno 1780.
—34 Reaumur's ſpirit of wine thermometers at Torneao.
—40 Fahrenheit's experiment, and congelation of mercury; alſo nitrous acid cryſtallizes.
—69 Cold produced by Mr. M'Nab, at Albany Fort in Hudſon's Bay, in December 1784, by means of ſulphuric acid and ſnow.
—121 Siberian cold
—148 Braun's expt. } obſerved with a ſpirit of wine thermometer.

1049 or 1050, therefore, is the greateſt heat which Sir Iſaac Newton attempted to meaſure, ſo as to learn how far it exceeds other heats, the ſtrength of which is more eaſily aſcertained.

But the late Mr. Wedgewood, who publiſhed two papers in the Philoſophical Tranſactions, on the meaſurement of violent heats, has gone much farther, and has ſhewn great ingenuity in the contrivance of a method for comparing and diſtinguiſhing violent heats.

The principal mean which he employed is the contraction of the pureſt kind of clay in the fire.

The properties of the more pure and perfect kinds of clay, with reſpect to heat, are well known to be, *1ſt*, A power to reſiſt the moſt intenſe heat, without melting. *2d*, A diſpoſition to contract, in all its dimenſions, when it is heated ſtrongly; which contraction is always greater, the greater the heat is to which the clay is expoſed.

This ſingular property of fine clay, at firſt, makes it appear an exception to the general law of expanſion of bodies by heat, and contraction by cold. In reality, however, it is not an exception; for the general rule of expanſion by heat, and contraction by cold, is only applicable to bodies which ſuffer no permanent change in their compoſition or texture, by the action of heat. But a maſs of dry clay does ſuffer a change in its texture by the action of a ſtrong heat; it becomes more compact and hard. I once ſuppoſed that it ſuffered a change in its compoſition, by evaporation of its volatile parts; but Mr. Wedgewood has ſhewn that this only happens in the beginning of ignition.

The contraction which happens afterwards muſt be only from a change of texture, a cloſer arrangement and contact of parts, which is permanent, and gives great hardneſs. Query, How does heat produce this change of texture? Heat may be ſuppoſed to produce it, by ſoftening the clay, and thus diſpoſing the particles of it to enter, by their attraction, into cloſer contact. A maſs of clay, ſimply dried, or made ſimply red hot, is exceedingly porous, and imbibes water remarkably. The particles of it have not a cloſe contact or connection with one another. It may be compared to dry clay in looſe powder, which we know becomes far more compact and denſe, by being ſoftened with water, and then allowing it to dry again. *(See Note* 11. *at the end of the Volume.)*

In whatever manner heat increaſes the denſity and hardneſs of clay, this effect of the heat is permanent, and remains after the heat is gone again; and a maſs of clay, thus changed, is never more affected by any inferior heats in a different manner from other bodies. In theſe inferior heats, it is expanded, and by cold contracted, like other bodies. It is only by expoſing it to a more violent heat than it has ſuffered before, that any further permanent contraction can be produced.

Of this property, therefore, Mr. Wedgewood takes advantage, in his method of meaſuring violent heats.

He forms a choſen, and very pure clay, into ſmall, ſhort cylinders,

which, by means of inſtruments properly contrived, are made exactly of the ſame ſize. The cylinders are then baked with a low red heat, to expel the air and water, which are contained in raw clay. Being thus prepared, they are ready for the meaſurement of ſtrong heats, of which they will give information in a few minutes. That time is ſufficient for penetrating them thoroughly with the heat to which they are expoſed; and if they be then taken out and cooled, they are always found contracted, more or leſs, in their dimenſions, according to the intenſity of the heat to which they were expoſed. This contraction being but a very ſmall quantity, the contrivance of a micrometer is neceſſary for ſubdividing it. To accompliſh this, the clay piece is introduced between two braſs rulers, firmly ſcrewed to a plate, and ſet, with a very ſmall inclination, to each other. Suppoſe them $\frac{1}{10}$ of an inch farther aſunder at one end than at the other, and that they juſt admit the clay cylinder, after its due preparation, at the wide end: And, ſuppoſe that the clay-piece has been in a pottery furnace, and that it is now admitted to the 75th diviſion of the rulers (the whole length being divided into 100 equal parts): We learn, by this experiment, that the cylinder has contracted $\frac{75}{100}$ of $\frac{1}{10}$ of an inch, or has contracted $\frac{75}{1000}$ of an inch, or $\frac{3}{40}$ of an inch.

Mr. Wedgewood's peculiar views made him leſs ſolicitous about the inferior heats, and he reſolved to begin this ſcale at the temperature called a full red heat. In order to give numbers to his ſcale, which ſhould convey ſome familiar ideas, he took the melting heat of fine gold for a fixed temperature, and by continual biſection, divided it into 32 parts, which he made the degrees of his ſcale.

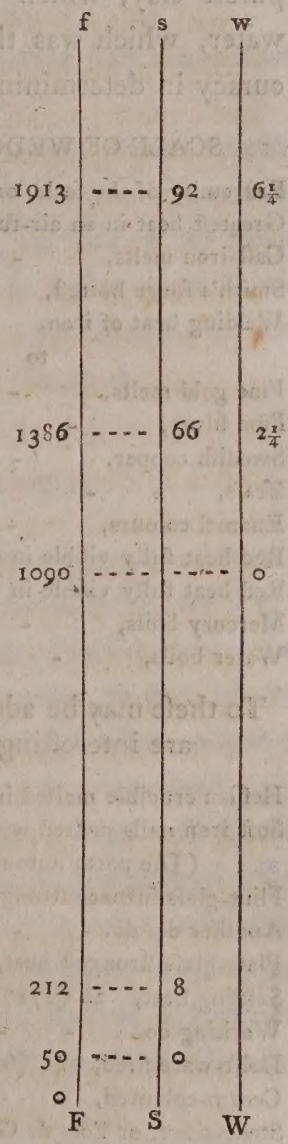
By this ſcale he aſcertained the temperature of ſeveral chemical proceſſes of great importance in the arts and manufactures.

The higheſt of theſe extended to 240° of his ſcale of heat.

But this gives us little information as to the relation which thoſe increments of temperature bear to thoſe which we have aſcertained by means of the common thermometer, even ſuppoſing the temperature

of full red heat, (the o of Mr. Wedgewood's ſcale) to have been well determined, which is a matter of ſome uncertainty. We do not know, from the proceſs now mentioned, how many degrees of Fahrenheit's ſcale correſpond to one of Wedgewood's.

Mr. Wedgewood connected his ſcale with Fahrenheit's in the following manner: The heat which raiſed Fahrenheit's thermometer from 50° to 212°, expanded a piece of ſilver from 0° to 8°, of a certain ſcale. A heat, which expanded this piece of ſilver from 0° to 66° of the ſilver ſcale, correſponded to $2\frac{1}{4}$° of the Clay or Wedgewood's ſcale, and 92° of the ſilver ſcale correſponded to $6\frac{1}{4}$° of Wedgewood's ſcale. Therefore, let the lines F f, S s, W w, parallel to each other, repreſent Fahrenheit's, the ſilver, and Wedgewood's ſcale. Fahrenheit's ſcale being divided into its degrees, draw through 50 and 212 the horizontal lines, cutting S s in 0°, and in 8°. Dividing 0-8 into eight equal parts, and carrying forward thoſe diviſions, draw lines through 66 and 92, cutting the two lines F f and W w. In Fahrenheit's ſcale, theſe lines will fall on 1386 and 1913, and on Wedgewood's ſcale they give $2\frac{1}{4}$ and $6\frac{1}{4}$, from which we readily find the place o to correſpond to 1090 of Fahrenheit. Mr. Wedgewood means his ſcale to commence with the heat given to iron by a ſtrong fire, and reckons this 1077 of Fahrenheit, differing from this conſtruction about $\frac{1}{10}$ of his degree, which is nearely 13° of Fahrenheit. (See Phil. Tranſ. vol. 72.) But I am by no means certain that the contractions of clay are proportional to the heats; but although they are not, this ingenious contrivance is



ſtill fitted for giving us very uſeful information concerning thoſe intenſe heats. Such heats certainly induce chemical changes; ſuch as tendencies to vitrification, or even this contraction obſerved in the pureſt clay, which may affect the heat communicated by them to water, which was the only other method which promiſed much accuracy in determining their temperatures.

SCALE OF WEDGEWOOD's THERMOMETER.		FAHRENHEIT's.
Extremity of the ſcale, or higheſt temperature obſerved, -	240	32277
Greateſt heat in an air-furnace eight inches ſquare, -	160	21877
Caſt-iron melts, - - - - - -	130	17977
Smith's forge hotteſt, - - - - -	125	17327
Welding heat of iron, - - - - -	95	13427
to - - - - -	90	12777
Fine gold melts, - - - - - -	32	5237
Fine ſilver, - - - - - -	28	4717
Swediſh copper, - - - - - -	27	4587
Braſs, - - - - - - - -	21	3807
Enamel colours, - - - - - -	6	1857
Red heat fully viſible in day-light, - - - -	0	1077
Red heat fully viſible in the dark, - - - -	0	947
Mercury boils, - - - - - -		600
Water boils, - - - - - - -	0	212

To theſe may be added ſome other temperatures, which are intereſting to many claſſes of inquirers:

Heſſian crucible melted in an iron furnace, at - -	150	20577
Soft iron nails melted with the bottom of the crucible, -	154	21097
(The parts untouched by the nails remained unaltered)		
Flint-glaſs furnace ſtrongeſt heat, - - -	114	15897
Another do. do. - - - - -	70	10177
Plate-glaſs ſtrongeſt heat, - - - -	124	16807
Settling heat, 28 (4717) or - - - - -	29	4847
Working do. - - - - - - -	57	8480
Delft-ware fired, 40 (6277) or - - -	41	6507
Cream-coloured, - - - - -	86	12257
Stone-ware, or Pots de Gres, - - -	102	14337
Worceſter china vitrified, at - - -	94	13297
Mr. Sprimont's Chelſea china, - - -	105	14727
Derby do. - - - - - -	112	15637
Bow do. - - - - - -	121	16007
Briſtol china, no vitrification, at - - -	135	18627

Common Chineſe do. no vitrification with any heat, but ſoftened and ſunk down at 130°.

True ſtone nankeen continues bibulous to the laſt.

Dreſden is more refractory than common china. Cream-coloured or queen's-ware bears heat as well as the Dreſden.

To melt equal parts of chalk and clay, which Pott ſays is a maſterpiece of art, requires 123.

From a general compariſon of all theſe obſervations, we ſee that the greateſt range of obſervations which have been made by means of Fahrenheit's thermometer, does not exceed $\frac{1}{32}$ of that of the obſervations made by Mr. Wedgewood, and which daily occur in the practice of the arts. It is alſo evident, that by a proſecution of ſuch obſervations, much valuable inſtruction may be obtained.

SECT. V.—OF INFLAMMATION.

THIS is the phenomenon familiarly known by the name of BURNING, or COMBUSTION. It obtains in ſuch bodies as take fire, or are kindled in a part, by the application of a very hot body, or otherwiſe, and continue to give out light and heat to other bodies, till they are burnt all over, or their faculty of burning is exhauſted, which we expreſs in common language by ſaying that they are conſumed.

Seeing that all bodies will not kindle and burn, as is well known, it may be thought improper to include this among the general effects of heat. But it is very general, and happens in many caſes where we do not attend to it, ſuch as lead, iron, and other metals; nay, even in the diamond. But my chief reaſon is, that an acquaintance with the diſtinguiſhing phenomena of combuſtion, is indiſpenſably neceſſary for our farther progreſs.

With theſe views, I obſerve, in the firſt place, that no natural operations, nor any experiments, ſhew us that thoſe ſubſtances which do not burn, either ſuffer any permanent changes in their principal

qualities, as ſubjects of chemical examination, by being thus heated, or afterwards give out more heat than they had received. They readily receive heat, and tranſmit it to others; but without either increaſing or diminiſhing the quantity of that heat, ſo far as we can perceive, or being themſelves altered in their principal qualities, in conſequence of having received it and loſt it again. Thus, water, or any pure ſaline or earthy ſubſtance, readily receives any quantity of heat to which it is expoſed. It will be expanded, and perhaps may ſuffer a change from the ſtate of a ſolid to that of a fluid, or from the ſtate of a fluid or ſolid to that of vapour. Or, if it be a fixed ſubſtance, it may be ignited, and will preſerve its new form and appearance as long as the heat continues; but, if it be placed beſide a colder body, it will quickly loſe its ſuperiority of heat, and, at the ſame time, return to its former condition in other reſpects. The vapour of water or ſalt, returns again to water or ſalt. And a ſalt or earth, after receiving ſo much heat as to be ignited, or perhaps melted, returns, in cooling, to the ſame ſtate as before; at leaſt it is not altered in its principal properties. It is ſtill a ſalt, or an earthy ſubſtance, while, at the ſame time, the heat which it had received, is tranſmitted to other bodies, without increaſe or diminution, that can be diſtinctly obſerved.

But ſuch is the nature of inflammable ſubſtances, that they are different from others in theſe two particulars. For, if they be heated to a proper degree in the open air, they not only continue hot, but become hotter; and this, in ſome circumſtances, to a violent degree of intenſity. And they continue in this ſtate, called the State of INFLAMMATION, for a longer or ſhorter time, during which they ſend out of them, into the ſurrounding matter, a continued, and often very copious, ſtream of heat and light, the whole of which, it is plain, they have not received in the uſual manner, or by communication from other bodies.

Further, this flow of heat comes to an end at laſt, and then we

find that the inflammable body has undergone a moſt remarkable change. It has loſt the qualities by which it was diſtinguiſhed as an inflammable body. It cannot now be again made a ſource of heat, nor is it qualified any longer to exhibit the phenomena I have deſcribed, but is on the ſame footing with other uninflammable matter, with reſpect to heat.

Such is, therefore, the general nature of the inflammable ſubſtances, and the diſtinction between them and other bodies. There are ſome of them which, to a careleſs obſerver, may ſeem, in ſome reſpects, exceptions to this general account; which, during inflammation, inſtead of being changed into a new ſort of matter, appear to be totally ſpent and conſumed, ſo that nothing remains in their place. Such is ſpirit of wine. Dr. Boerhaave conſiders it as a pure pabulum of fire. Such alſo is ſulphur. But the appearances here are deceitful. Spirit of wine is changed by inflammation into a great quantity of water—Sulphur into a great quantity of acid.

The matters produced from the other inflammable ſubſtances are very various. From phoſphorus we obtain, by inflammation, a fixed acid; from others we obtain water, earths, ſalts of different kinds, and aerial matter; and ſome afford ſeveral of theſe ſubſtances at the ſame time. This is the caſe of the common animal and vegetable inflammable ſubſtances.

All the inflammable ſubſtances, therefore, are changed, during their inflammation, into one or more principles, or kinds of matter, which are no longer inflammable.

Another particular, which muſt be attended to in conſidering the general nature of inflammation, and of the inflammable bodies, is the neceſſity of air to inflammation.

I began this ſubject with ſaying that the inflammable ſubſtances enter into a ſtate of inflammation, and ſuffer the change deſcribed, when heated to a certain degree in the *open air*. The being heated to a certain degree, in the firſt place, is a circumſtance undoubtedly

neceſſary to the beginning of inflammation, though the degree or intenſity of heat neceſſary is very different, in the different kinds of them. More or leſs heat, however, is neceſſary in all caſes; and I have choſen, therefore, to introduce this ſubject of inflammation among the effects of heat.

But it is equally neceſſary that the body be expoſed to the action of air, and even of freſh air, conſtantly renewed at its ſurface. If we attempt to inflame a body in a ſpace exhauſted of air, we never ſucceed. If we even attempt it, in air that is confined, the inflammation ſoon ſtops, although the inflammability of the burning body be not exhauſted. The air with which it was incloſed undergoes a change, which renders it unfit any longer to continue the inflammation which it promoted at firſt *.

Theſe are the chief facts relating to this ſubject, and which have been remarked for a long time. It will be proper, now, to attend a little to the opinions which have been formed concerning the nature and cauſes of theſe phenomena.

The opinion which was firſt formed among the chemiſts, and was received as juſt and adequate for a conſiderable time, was this:

It was imagined that the quality of inflammability depended on a principle, or material ſubſtance, which they ſuppoſed to be contained in all the inflammable ſubſtances, and to be the ſame in all the different kinds of them; and that the diverſity among them, in external appearance, and other qualities, proceeded from the other principle, or number of principles, with which this common principle is combined; and the chemiſts conſidered inflammation, with the ſeveral

* There are, indeed, a few caſes where bodies will burn, and be conſumed, in the cloſeſt veſſels. This you all know to happen with gunpowder, and all ſubſtances containing ſaltpetre, and any ordinary combuſtible. But chemiſtry teaches us that this is no exception, and that the ſaltpetre is decompoſed by the heat and the combuſtible body, and yields a continual ſupply of the pureſt air.

EDITOR.

phenomena that attend it, as depending on a gradual ſeparation and diſſipation of this common principle, the φλογιστον, which being once ſeparated, what remained of the body could no longer be an inflammable ſubſtance, but muſt be ſimilar to the other kinds of matter.

This opinion was founded chiefly on ſome facts and experiments, of which it is proper to give you a general knowledge. There are numerous experiments and operations in chemiſtry, whereby the uninflammable matter, into which ſome of theſe bodies are changed during their inflammation, can be reſtored again to its former ſtate, and rendered again capable of being inflamed. We have noted examples of this in ſulphur and in phoſphorus. Both of theſe inflammable ſubſtances are changed, by inflammation, into acids, the one of which is called the ſulphuric acid, or vitriolic acid, and the other the phoſphoric acid. But each of theſe acids can be again reſtored to the ſtate of an inflammable body; the one to the ſtate of ſulphur, the other to that of phoſphorus, as before: And this reſtoration is effected by mixing them with charcoal duſt, and expoſing this mixture to a ſtrong heat. The charcoal employed, or a part of it, is conſumed, and diſappears, while the acid reſumes the form of an inflammable body. The diſappearance or diminution of the charcoal in theſe operations, made the chemiſts imagine that a part of it was combined with the acid matter, and brought it back to the ſtate of an inflammable ſubſtance, by reſtoring to it ſome principle which it had loſt during its own production from the inflammable ſubſtance, by inflammation.

This ſuppoſition having been formed, certain variations of theſe experiments were thought to prove that this ſuppoſed principle is the ſame in all the inflammable bodies; for, in reſtoring the acid of ſulphur to the ſtate of ſulphur again, or the acid of phoſphorus to the ſtate of phoſphorus, we are not limited to the uſe of any one kind of charcoal, or indeed any one kind of combuſtible matter, if it be ſufficiently fixed to bear the neceſſary heat, without eſcaping in volatile fumes.

Upon theſe facts, therefore, was founded a perſuaſion that the inflammable ſubſtances contain a principle which gives them the quality of inflammability, which they have in common, and which principle the chemiſts called phlogiſton, aſſerting that it is preciſely the ſame in all the ſpecies belonging to this claſs, and that it is diſſipated from them during their inflammation, but can be again reſtored by proceſſes like thoſe I have juſt now deſcribed.

But when we inquire further, and endeavour to learn what notion was formed of the nature of this principle, and what qualities it was ſuppoſed to have in its ſeparate ſtate, we find this part of the ſubject very obſcure and unſatisfactory, and the opinions very unſettled.

The elder chemiſts, and the alchemiſts, conſidered ſulphur as the univerſal inflammable principle, or at leaſt they choſe to call the inflammable part of all bodies, that are more or leſs inflammable, by the name of their ſulphur, which they appear to have conſidered as a general principle of inflammability. The famous German chemiſt, Becher, was, I believe, the firſt who rejected the notion of ſulphur being the principle of inflammability in bodies. He conſidered ſulphur as one of the inflammable ſubſtances, which contained the principle of inflammability like the reſt, but which was not the pure principle of inflammability itſelf.

His notion of the nature of the pure principle of inflammability was afterwards more fully explained and ſupported by Profeſſor Stahl, who, agreeably to the doctrine of Becher, repreſented the principle of inflammability as a dry ſubſtance, or of an earthy nature, the particles of which were exquiſitely ſubtile, and were much diſpoſed to be agitated, and ſet in motion with inconceivable velocity. You have a full account of this explanation in his *Theoria Chemiæ Dogmaticæ*, a work of great merit and ingenuity, and which was long regarded as the infallible code of chemical ſcience.

The opinion of Becher and Stahl concerning their *Terra Secunda*, or *Terra Inflammabilis*, or *Phlogiſton*, was, that the atoms of it are,

more than all others, diſpoſed to be affected with an exceſſively ſwift whirling motion, *motus vorticillaris*. The particles of other elementary ſubſtances are likewiſe liable to be affected with the ſame ſort of motion, but not ſo liable as theſe of the *Terra Secunda*: And, when the particles of any body are agitated with this ſort of motion, the body exhibits the phenomena of heat, or ignition, or inflammation, according to the violence and rapidity of the motion. This notion of heat, as depending on a motion or agitation induced in the particles of bodies, was founded on the common obſervations and concluſions, firſt inferred by Lord Verulam, from the conſideration of the different means by which heat may be produced or excited in bodies. Becher and Stahl, therefore, did not ſuppoſe that heat depended on the abundance of a peculiar matter, ſuch as the matter of heat or fire is now ſuppoſed to be, but on a peculiar motion of the particles of matter; and that their phlogiſton, which was ſuppoſed to be contained in all the inflammable ſubſtances, was, moſt of all other matter, diſpoſed to aſſume this ſort of motion. *Vide Juncker, vol.* 1. *Tab. de Principiis, et Tab. de Inſtrumentis Chemiæ eſſentialibus.*

This very crude opinion of the earthy nature of the principle of inflammability appears to have been deduced from a quality of many of the inflammable ſubſtances, by which they reſiſt the action of water as a ſolvent. The greater number of the earthy ſubſtances are little, or not at all, ſoluble in water; and ſome time ago they were reckoned, when in their pure ſtate, perfectly inſoluble. And, as Becher and Stahl found thoſe compounds, which they ſuppoſed contained phlogiſton in the largeſt quantity, to be inſoluble in water, although the other matter, with which the phlogiſton was ſuppoſed to be united, was, in its ſeparate ſtate, exceedingly ſoluble in that fluid, they concluded that *a dry nature, an incapability to be combined with water*, was an eminent quality of their phlogiſton; and this was what they meant by calling it an earth, or an earthy ſubſtance.

They thought it was moſt abundant, and leaſt clogged with mat-

ter in charcoal, and in ſoot, both of which are very inflammable, and inſoluble in water, and, in ſome meaſure, ſhew even a repulſion for that fluid. The fineſt kind of ſoot (lamp black) is very difficultly wetted with water.

But theſe authors ſuppoſed, at the ſame time, that the particles of this dry and earthy phlogiſton were much diſpoſed to be exceſſively agitated with a whirling motion; which whirling motion, exerted in all directions from the bodies in which phlogiſton is contained, produced the phenomena of inflammation.

This appears to have been the notion formed by Becher and by Stahl, concerning the nature of the principle of inflammability, or the phlogiſton; a notion which ſeems the leaſt entitled to the name of explanation, of any thing we can think of. I preſume that few perſons can form any clear conception of this whirling motion, or, if they can, are able to explain to themſelves, how it produces, or can produce, any thing like the phenomena of heat or fire.

Succeeding chemiſts, however, refined afterwards upon this notion, and formed an opinion which had much more appearance of probability.

This opinion was founded in the phenomena of inflammation, and on ſome new facts relating to this ſubject, which Becher and Stahl had not attended to, or had not the opportunity of obſerving.

When a combuſtible body is ſet on fire, and is undergoing the change which I have deſcribed, what appears the moſt obviouſly, and evidently, to be ſeparated from it, and to be thrown off, is heat and light. But, from a great number of conſiderations, there is reaſon to conclude, that heat and light depend on the motion and action of an inconceivably ſubtile, penetrating, and elaſtic matter, the particles of which are minute beyond what the utmoſt efforts of our imagination can conceive, and have a ſtrong repulſion for one another; and thus are capable of being thrown off from bodies, or ſet in motion, with a velocity that is aſtoniſhing. It has been proved, by aſtronomical

obſervations, that light comes from the ſun to us in 8′. 13″. A cannon ball, flying with the velocity of 600 feet per ſecond, would require more than 25 years to go to the ſame diſtance. The velocity of light, therefore, is perfectly aſtoniſhing, and would enable it to beat into powder, and diſperſe in duſt, every object on which it ſhines, were not the particles of it ſo inconceivably fine and minute, that they are thereby deprived of this deſtructive power. Their minuteneſs is ſuch, that ſome have demonſtrated, that 200,000,000,000 of them, at leaſt, can paſs through a pin-hole made in a card, croſſing one another in every direction, and in the ſame moment of time, or at leaſt in a time ſo ſhort, that it appears indiviſible.

This exquiſitely ſubtile and active matter, therefore, was, by the later chemiſts, ſuppoſed to be capable of being fixed and combined with other matter, ſo as to enter into the compoſition of certain bodies; and, in this combined and fixed ſtate, gave to ſuch bodies the quality of inflammability, and was ſeparated from them during their inflammation.

This is the opinion I had of it formerly, before the exiſtence of a phlogiſton, or principle of inflammability, began to be doubted. And Mr. Macquer, and other chemiſts, alſo formed the ſame opinion.

One principal foundation of this opinion was a number of experiments and obſervations, from which it appears that certain bodies, not inflammable, are changed into inflammable bodies, by ſimply expoſing them to the light of the ſun; or, if they be not actually changed into inflammable bodies, they ſuffer the ſame changes as thoſe they ſuffer, when we apply to them other inflammable ſubſtances, with the intention to communicate to them the quality of inflammability, or other qualities which are connected with it.

We have an example of this, in the production of the green matter of the leaves of plants. It has long been known, that the green colour of the leaves of vegetables is produced by the light of the ſun. The moſt direct experiments to aſcertain this fact, were made firſt by Mr. Du-

fay, and others of the French academicians; and the inveſtigation has been ſince carried farther by Mr. Sennebier of Geneva, and others. When ſeeds are ſown in a dark place, the plant grows luxuriantly, but with a foliage extremely different from what is common to it, and entirely without the green colour. This is the caſe, although plenty of freſh air be admitted to them. When the plant is in this ſtate, if the light be admitted, it immediately begins to acquire a green colour, which in time arrives at its ordinary fulneſs. What is farther remarkable in this experiment, is, that the plant directs its growth to the light, although the hole by which this is admitted be cloſed up by a piece of glaſs.

Profeſſor Robiſon informed me, that he ſaw tanſy growing in a coal-pit, with a very abundant foliage, but white, and without ſmell; and that, when the clod was brought above ground, this ſubterranean growth died down, and was ſucceeded by freſh leaves of a full green, and ſtrong aromatic ſmell. Hence he concluded, that the ſun's rays not only produceth the green fæcula, but alſo the eſſential oil of plants. Hence we may account for the deep green, and the great abundance of aromatic plants, ſhrubs, and trees, in the warm climates. *(See Note* 12. *at the end of the Volume.)* Upon this depends the art of blanching endive, celery, and many other garden plants, for the uſe of the table, in order to have them tender and ſweet, and free from any ſtrong biting taſte. We bind up their leaves as they ſprout, keeping them from the light. Some eſculent vegetables produce ſuch a tuft of leaves, and theſe ſo broad and compacted, that the external leaves alone get the benefit of the light, and are coloured green by it. Such vegetables, therefore, ſcarcely require the aid of art to blanch them. Such are ſome kinds of lettuce and cabbage. It is quite plain, therefore, that the green colour of their leaves is produced by light. A long time ago, I had the curioſity to examine this green matter, to learn the nature of it. It ſeparates from the reſt of the matter of the plant, when that is

maſhed and macerated with water. It may be completely taken up by ſpirit of wine. This fluid diſſolves the green fæcula completely, and may afterwards be expelled from it by heat. It is highly inflammable, and burns completely, without leaving any ſenſible quantity of aſhes.

Thus have I briefly ſtated the opinions concerning the nature of inflammation, and of the inflammable bodies, which had been formed among the chemiſts until about fifteen or twenty years ago. Before that time, however, a fact had been obſerved with ſurpriſe that was very inconſiſtent with this opinion, viz. That ſome of the inflammable bodies, when changed by inflammation into uninflammable matter, afford a quantity of this matter of greater weight than the inflammable body itſelf. About the period I juſt now mentioned, or ſince that period, this fact has been better aſcertained, and diſcovered to be true with reſpect to all the inflammable bodies, and in all caſes of inflammation. The reaſon why the chemiſts were ſo late in diſcovering this fact, with reſpect to a great number of theſe caſes, was, that the uninflammable matter produced is, in many of the experiments, a volatile ſubſtance, and eſcaped during the examination. But, ingenious methods having been deviſed for collecting and weighing the whole of this volatile matter, as well as the reſt of the uninflammable ſubſtances into which bodies are changed during the inflammation, it has been found, that in general, there is more of this uninflammable matter than there was of the body before it was inflamed. From an ounce of phoſphorus, for example, we may perhaps get an ounce and a half, or more, of the ſolid acid ſalt, into which it is changed by being inflamed.

This appeared to many an inſuperable objection to the theory of the chemiſts; and, though ſome attempts were made to obviate this objection, they reſted on ſuppoſitions ſo very ſubtile and difficultly comprehended, perhaps abſurd, that they were quite unſucceſsful.

Another theory, therefore, which I may ſay is the oppoſite or con-

trary to this, has lately been formed, and is faſt gaining ground, eſpecially on the continent. I cannot explain to you at preſent the whole of this theory, or trace the ſteps by which it has been brought to its preſent ſtate. We ſhall have better opportunities for doing this in other parts of the courſe, as you will have by that time become well acquainted with ſeveral ſubſtances, whoſe properties have a great influence in producing the phenomena which furniſh the chief arguments in ſupport of the theory. I can only remark juſt now, that it is founded on a very great number of experiments and diſcoveries, which have been made within theſe twenty years (1789) on the nature and qualities of atmoſpherical air, and of a number of other elaſtic fluids.

Ingenious men in different parts of Europe, incited by the remarkable properties which I had diſcovered in one of thoſe fluids in 1756, began to employ themſelves in ſuch experiments, and they have made many aſtoniſhing and moſt intereſting diſcoveries. The fundamental experiments were firſt made, and the leading inferences were firſt drawn in this country, by Dr. Prieſtley, the Honourable Mr. Cavendiſh, and my friend Mr. Watt. But it was chiefly in France that they were repeated, with proper attention to all the circumſtances that would affect the reſult, and this reſult was made the foundation of a new theory of combuſtion. Theſe experiments have been made there with extraordinary ingenuity and accuracy, and on ſo large a ſcale, that the effects produced were the more clearly and fully perceived. The unfortunate Lavoiſier, who fell a ſacrifice to the ambition of his philoſophical aſſociates, and whoſe unjuſt and cruel fate, and the loſs which ſcience has ſuffered by it, cannot be too much deplored, was the moſt active in this inquiry, ſparing neither labour nor expence. His own exertions diſplay the greateſt ingenuity. He alſo employed the talents of other eminent philoſophers in aſcertaining ſome of the moſt important and intereſting facts; but Lavoiſier himſelf was the principal author of the new theory.

It had long before been known, as I have already informed you, that air, and even a conſtant ſupply of freſh air, is neceſſary to inflammation; that if a burning body be ſhut up with a certain moderate quantity only of air, the inflammation ſtops after ſome time, although the inflammability of the body be not exhauſted. The incloſed air has undergone a change which makes it totally unfit for continuing the inflammation. In ſome of the experiments made by Dr. Hales, and other perſons, it had alſo appeared, that the air is generally diminiſhed on this occaſion, or, in the language of Dr. Hales, a part of it was abſorbed.

But this has been aſcertained with much more preciſion, and in a much greater number of caſes, by the later experiments. It has become clear and evident, by many of theſe, that a conſiderable quantity of the air is really abſorbed, and combined with the matter of the burning body, ſo as to form, in many caſes, a denſe compound, in which the air ſo abſorbed is totally deprived of its uſual form of an elaſtic fluid: And the additional weight which the matter of the burning body acquires, has been found to correſpond exactly to the weight of the air which has been abſorbed by it.

Farther, the two very different combinations of heat, which I had diſcovered, in which it produces fluidity and vapour, encouraged Mr. Lavoiſier to preſume that there was another combination, which produced a permanently elaſtic fluid, not decompoundable, like a liquid, or a vapour, by the touch of a colder body *; and the different capacities for heat, having been already diſcovered, it appeared no difficulty to account for the vaſt quantities ſet looſe in combuſtion. This new theory, therefore, is founded on the doctrine of latent heat, and is, indeed, an extenſion of it.

For, in the remarks I lately made to you on the production of

* This is not ſtrictly true, for the oxygenated muriatic acid gas is condenſed by a cold —32° of Fahrenheit.

EDITOR.

vapour from bodies by the action of heat, I made it evident, that vapour contains a very great quantity of heat, beſide that which we perceive in it by the ſimple application of the thermometer; and that this heat, which appears to be a neceſſary ingredient in the compoſition of the vapour, comes out of it again when it is condenſed. In Mr. Lavoiſier's theory, the ſame thing is ſuppoſed to happen during the action of air and burning bodies on one another. The matter of thoſe bodies attracts the matter, or baſis of the air, or rather of a part of the air, which is different from the reſt, and with which I ſhall make you hereafter better acquainted. The burning body collects, and condenſes this ſort of air, depriving it at once of its vaporous and elaſtic aerial form; or, if it does not abſolutely condenſe it to a ſolid matter, it diminiſhes, to a great degree, its capacity for heat. All, or the greater part, of that great quantity of heat, therefore, which was combined with the matter of this air, and thermometrically latent in it, is ſuddenly let looſe, to diffuſe itſelf among the ſurrounding bodies: And there is a continued emiſſion of it, in this manner, ſo long as the air and burning body continue to act on one another, or until that portion of the air, which is capable of being thus attracted, and compounded with the burning body, is totally expended. This theory, therefore, is directly contrary to the theory the chemiſts had formed before. *(See Note 13. at the end of the Volume.)*

According to this theory, the inflammable bodies, ſulphur, for example, or phoſphorus, are ſimple ſubſtances. The acid into which they are changed by inflammation is a compound. The chemiſts, on the contrary, conſidered the inflammable bodies as compounds, and the uninflammable matter as more ſimple. In the common theory, the heat and light are ſuppoſed to emanate from, or to be furniſhed by, the burning body. But, in Mr. Lavoiſier's theory, both are held to be furniſhed by the air, of which they are held to be conſtituent parts, or ingredients, while in its ſtate of fire-ſupporting air.

Such are the outlines of this celebrated theory of combuſtion. I

cannot enter on it in greater detail, in this part of the courſe, but this brief ſketch was neceſſary for the underſtanding what follows, concerning the management of fuel in our furnaces, and for ſome other purpoſes. The full conſideration of it will be given, when thoſe fuels are conſidered as ſubjects of chemical examination, by the help of doctrines depending on the chemical nature of other ſubſtances, of which the treatment is more elementary.

After theſe obſervations on the general proceſs and phenomena of combuſtion, I muſt explain to you ſome appearances, which, though not univerſal, yet obtain to a very great extent, and occaſion important differences in the phenomena, and in the employment of combuſtion, as an inſtrument of chemiſtry.

FLAME, and that particular form of combuſtion which is properly called INFLAMMATION, requires ſome particular conſideration. With this view, therefore, I muſt firſt remark that different inflammable ſubſtances have very different degrees of volatility. Some cannot be converted into vapour with any degree of heat which we can command. Theſe do not ſuffer any loſs from heat, if we preſerve them from the action of the air; and when the air is admitted, their combuſtion is attended with a gradual conſumption of their ſurface only. But many others are more or leſs volatile, and many ſo volatile, that they begin to be converted into vapour before we can heat them to the degree neceſſary for their inflammation. It is evident that we cannot inflame theſe, except in the form of vapour;—they are more volatile than inflammable. The uſual practice, therefore, is to heat them, or a ſmall part of them, until they emit vapour, and then, at the ſame time, to ſet that vapour on fire. Thus we produce FLAME, the heat of which occaſions more vapour to riſe, and to be inflamed, in a conſtant ſucceſſion, until the whole maſs has undergone inflammation. This is illuſtrated by examining attentively the flame of ſpirits of wine,

burning in a ſmall veſſel or diſh, $1\frac{1}{2}$ or 2 inches diameter. *(See Note 14. at the end of the Volume.)*

That the flame, even of oily ſubſtances, as oil, or tallow, is truly a tranſparent vapour, which appears otherwiſe in a dark place, in conſequence of its emitting heat and light, becomes evident, when we place a candle in the ſunſhine. The light of it being too weak to be perceived in the ſtrong light of the ſun, the flame is not ſeen. We ſee objects placed beyond it, as well as if it were not interpoſed; and when we obſerve the ſhadow of it on a wall, we ſee no ſhadow but that of the wick; the rays of the ſun paſs through the flame, and reach the wall without interruption. They are only a little refracted, bent from their ſtraight courſe, while they paſs through the rare vapour of which the flame is compoſed, or through the air heated by the flame; and therefore the ſhadow of the candle, when thrown on a white wall, ſhews an appearance of thin ſmoke ariſing from the wick.

GENERAL REFLECTIONS.

I have now finiſhed the account I propoſed to give of the more general effects of heat; in communicating which, I have followed the plan which appears to me the moſt proper to lead to improvements in our knowledge of this ſubject, and by following which, many improvements have actually been made.

One part of the ſubject, however, may appear to you to have been omitted; and it is a part upon which Dr. Boerhaave has beſtowed a great deal of labour and ſtudy. The part I now refer to is the different means by which heat may be produced, or increaſed, and even diminiſhed alſo; or the different means for producing heat, or cold.

But although I have not attempted, as Dr. Boerhaave has done, to bring together all the facts belonging to this part of the ſubject, I have taken occaſion to mention the greater number, and the principal of

thoſe facts. To have treated them as minutely as that author, would have taken up too much of our time at preſent; and we ſhall have a better opportunity hereafter to notice thoſe parts of it which remain untouched. But, from the progreſs we have already made in ſtudying the general effects of heat, I am perſuaded you will view the ſubject as one of the moſt curious and intereſting parts of the ſtudy of nature.

There is, perhaps, no inquiry more worthy of the attention of the philoſopher than the nature of heat, and the manner in which matter in general, and the different kinds of it, are affected by this wonderful agent. Its influence is manifeſtly ſo univerſal, and its action ſo important and neceſſary to the progreſs of all the operations of nature, that, to thoſe who conſider it with ſome attention, it will appear to be the general material principle of all motion, activity, and life, in this globe. Heat is inſeparably neceſſary to the very exiſtence of vegetables and animals. Without heat, plants cannot live. Without it, they want the power to attract their nouriſhment, or to ſet it in motion through their ſyſtem, or to refine and ripen it in their different parts. Their vigour and life depend on its influence. It is only when enlivened by heat, that they make it aſſume the various forms and qualities which we find in the wood, the root, the leaves, the juices, the fruit, the ſeeds; and the beautiful forms and colours diſplayed in the flowers. They decay and die when heat departs. Nor is animal life leſs immediately dependent on heat, for ſupport, than vegetable. Heat is the main ſpring in the corporeal part of an animal, without which all motion and life would inſtantly ſtop. There are few facts more unaccountable than the effect of heat on an egg, though there are few to which we pay ſo little attention. We ſee a lump of apparently dead matter, which, left to itſelf, would continue dead and inactive for ever. By the application of a gentle degree of heat, it ſoon has an animal formed within it, which quickly increaſes in ſize and perfec-

tion, until it breaks open its incloſure, perfect in all its parts, and ready to perform its proper functions.

The effect of the heat, upon this occaſion, does not depend upon the particular manner in which it is produced or applied. In moſt caſes, it is ſupplied by the mother; but it is well known, that in a very great number, no heat is neceſſary but that of the ſun: And art has ſucceeded in employing the heat of fuel, and that produced by maſſes of fermenting or putrefying matter.

In the incubation of an egg, therefore, we ſee an example of an animal brought into exiſtence by heat, operating as a circumſtance neceſſary or indiſpenſable to its production. But, after the animal is thus brought into exiſtence, heat is ſtill neceſſary for its ſupport. If heat be diminiſhed to a certain degree, although no viſible damage be produced, all motion and life are quickly extinguiſhed. The animal is ſeized with a ſleep and inſenſibility, under which it expires. This is well known in the countries where the colds of winter are violent; and the inhabitants are upon their guard to ſhake off the firſt appearance of drowſineſs, produced by exceſſive cold. There are, it is true, ſome kinds of animals, as ſwallows, bats, frogs, &c. to which this ſort of ſleep is not fatal; that is, it does not deſtroy the ſeeds of life ſo effectually as to take away the poſſibility of its returning. But even this ſleep, when it continues, is attended with all the ſymptoms of death, except putrefaction, and nothing but the return of heat can reſtore to the animal its life and activity.

Thus, therefore, we ſee that the whole of the vegetable creation, and all the varieties of animal life, depend on heat for production and ſupport, or cannot be produced and ſupported without it. How wonderful and extenſive, therefore, in its operation and influence, as confined within limits ſet to it by the Author of nature, and as ſubject to the gentle and regular viciſſitudes of day and night, ſummer and winter!

It is plain, that not only all animal and vegetable life, but that the whole face and appearance of nature, the very form and powers of the elements themſelves, depend on this limited action of heat. There are none of the elementary bodies with which we are better acquainted than water. Let us attend a little to the powers and qualities by which it acts its part in this ſyſtem of beings. We all admire its pure tranſparency in a ſpring; the level and poliſhed ſurface with which it reflects objects that are on the banks of a lake; the mobility with which it runs along the channel of a brook, and the inceſſant motion of its waves in a ſtormy ſea. But, when viewed with a philoſophical eye, it appears much more an object of admiration. The ſame water, which, under its uſual form, is ſuch a principal beauty in the ſcene of nature, is employed in her moſt extenſive operations, and is neceſſary to the formation of all her productions. It penetrates the interior parts of the earth, and appears to aſſiſt in the production of various minerals, ſtones, and earths, found there, by bringing their different ingredients together, and applying them to one another properly, that they may concrete. We know it ariſes in vapours from the ſurface of the ocean, to form the clouds, and to deſcend again in rain upon the dry land, and give origin to ſprings, rivers, and lakes; or, upon proper occaſions, to form deep ſnow, which protects the ground and vegetables from the intenſe and mortal cold to which ſome parts of the world are expoſed; and, after it has performed this uſeful office, it readily yields to the heat of ſummer, and returns to a ſtate in which it ſerves the ſame purpoſes as rain. By its fluidity and tenuity, it penetrates the ſoil, and the ſeeds of plants which that ſoil contains. Theſe it cauſes to ſwell and germinate into plants, which depend on water for ſupport. It paſſes with freedom and eaſe through all their minuteſt tubes and veſſels, and carries with it materials neceſſary for nouriſhment and growth, or changes its appearance ſo as to become part of the plant. There is no plant or vegetable ſubſtance,

that does not contain in its compoſition a large quantity of water, eaſily ſeparable from it. The hardeſt woods contain a great deal. The ſofter and more ſucculent parts of vegetables are almoſt totally compoſed of it. Even the oils and reſinous ſubſtances can be reſolved in part into water. It is plainly as neceſſary to the animals, and is found to be as copious an ingredient in the compoſition of their bodies, and of all the different parts of them.

Theſe are the numerous and extenſive uſes of this beautiful element. But, in this ſucceſſion of forms and operations which it undergoes, you will perceive that it is ſet in motion, and adapted to theſe ends, by the nice adjuſtment and gentle viciſſitudes of heat and cold, which attend the returns of day and night, and ſummer and winter; and that even the *form*, under which it and the other elements play their parts, depends on the limited action of heat. Were our heat to be diminiſhed, and to continue diminiſhed, to a degree not very far below the ordinary temperature, the water would loſe its fluidity, and aſſume the form of a ſolid hard body, totally unfit for the numerous purpoſes which it ſerves at preſent. And, if the diminution of heat were to go ſtill farther, the air itſelf would loſe its elaſticity, and would be frozen to a ſolid uſeleſs matter like the water; and thus all nature would become a lifeleſs, ſilent, and diſmal ruin. Such being the important part allotted to water, in the magnificent ſeries of natural operations, in conſequence of the qualities communicated to it by heat, all its properties become intereſting objects of contemplation to a ſenſible heart. That peculiarity, by which the expanſion and contraction of water by heat is diſtinguiſhed from the ſame effect on other ſubſtances, I mean its irregularity between 32° and 40° of Fahrenheit, naturally attracts attention. Even this ſeemingly trifling diſtinction has been ſhewn by Count Rumford to have a mighty effect in rendering our habitation more comfortable.

On the other hand, were the heat which at preſent cheriſhes and

enlivens this globe, allowed to increaſe beyond the bounds at preſent preſcribed to it; beſide the deſtruction of all animal and vegetable life, which would be the immediate and inevitable conſequence, the water would loſe its preſent form, and aſſume that of an elaſtic vapour like air; the ſolid parts of the globe would be melted and confounded together, or mixed with the air and water in ſmoke and vapour; and nature would return to the original chaos.

Such are the objects and reflections which preſent themſelves to our minds, when we contemplate the general effects of heat in the univerſe: And with theſe I now finiſh the firſt ſection of the firſt part of our courſe.

PART II.

GENERAL EFFECTS OF MIXTURE.

INFORMED as you now are of the general effects of heat, your minds will be greatly assisted in your endeavours to form distinct conceptions of the results of the mixture of different substances. This, you know, is the other great mean of accomplishing the chemical changes in nature or art.

Upon this subject we have not so many general facts to communicate as on the subject of heat. The effects of mixture are so extremely diversified, that it is more difficult to find such as occur in a great number of cases, or which can be considered as general effects. Yet there are some of this kind; and it is proper to take notice of them here, that we may have an opportunity to become acquainted with the bond of chemical union, which is the characteristic of our science, and from our notions of which are principally derived all our attempts to explain the chemical phenomena of nature, or to reason concerning them.

It is therefore chiefly with a view to prepare you for understanding the principles by which many of the chemical phenomena are considered as explained, that I am now to lay before you the more general facts relating to mixture; and they are these:

1. In the firſt place, we have learned, by innumerable experiments with the various kinds of bodies, that, though ſome, when mixed together, or applied to one another with a view to mixture, cannot be made to unite; as oil with water, water with mercury, ſand with water;—others readily unite in the moſt intimate manner, and form *compounds*, which have, however, all the appearance of being ſimple and homogeneous, or in which the eye cannot diſcover, even with a microſcope, the ſmalleſt ſigns of any diverſity in the conſtituent parts.

2. In ſome of theſe caſes again, this intimate union is effected ſlowly and quietly, and is not accompanied by any other phenomena that are very remarkable; as, when pure common ſalt is diſſolved in water into a clear brine; when ſugar is diſſolved into a ſyrup, or when vinous ſpirits are diluted in water. But, in other caſes, the ſubjects unite with an appearance of violence, or with rapidity, commotion, and the production of heat. Of theſe caſes you will have more diſtinct knowledge by ſome examples.

Firſt Example. If equal meaſures of vitriolic acid and water (as eight ounces) be poured into a Florence flaſk, we obſerve that the intimate union is performed with rapidity, commotion, and the production of heat. The heat produced from the ſudden mixture of this quantity of ſtrong acid and water, is ſo great, that it will ſplit the glaſs veſſel, unleſs extremely thin.

Second Example. Another inſtance may be taken, where the appearance of violence and commotion is ſtill greater, though the heat produced is not ſo conſiderable. Put into a tall glaſs a ſmall quantity (ſuppoſe four ounces) of the common ſpirit of hartſhorn, called by the chemiſts mild volatile alkali, united with water, and pour into it moderately ſtrong vitriolic acid, by ſmall quantities at a time: The liquor inſtantly boils up, foaming and frothing, and ſputtering up many ſmall drops. This turbulent motion ceaſes after a little while, but is renewed by the next addition of acid. After a few ſuch additions, the foaming becomes leſs remarkable, but will increaſe by agitat-

ing the veſſel, or by ſtirring the mixture. After ſome time, no more of it appears. We have now produced a mixture perfectly tranſparent, and uniform in all its parts.

The appearances of turbulent union, and moſt violent action of the ſubſtances on each other, are ſtill more remarkable in ſome caſes; and even ſmoke and flame ſometimes burſt out with violent exploſion.

I may take this opportunity to remark, that this ſort of violent and turbulent inteſtine motion, and foaming, which accompany the union of different bodies, is called EFFERVESCENCE, among the chemiſts, and that the bodies are ſaid to EFFERVESCE with one another.

3. Theſe examples are caſes of the union of fluids with one another. There are many others, which, at the firſt view, may perhaps appear more ſurpriſing, in which even ſolid, and ſometimes very hard bodies, when thrown into certain liquids, begin immediately to be torn aſunder, with more or leſs rapidity and violence, into atoms ſo minute as to become entirely imperceptible to our ſenſes; in which ſtate of exquiſitely ſubtile diviſion, they are diſperſed through every part of the fluid, and compoſe, with it, an apparently homogeneous liquor. They form, with the fluid, a liquor which is not only quite tranſparent, and, in appearance, pure and ſimple at firſt, but which, though kept for ages, will remain ſo, without depoſiting the ſmalleſt ſediment, or ſhewing any other ſigns of its being compounded. This happens, too, in many caſes, although the ſolid be compoſed of much heavier matter than the fluid, and ſink in it, at firſt, as a ſtone in water. Notwithſtanding its ſuperior weight, it is ſoon combined with the fluid, in the manner I juſt now mentioned, and the particles of it never afterwards ſeparate or ſubſide to the bottom, but will remain for ever equally diſperſed through every part of the fluid, and cloſely and intimately united with it.

This happens to different ſolid bodies, with more or leſs rapidity, and appearance of violence. As examples, therefore, of ſuch com-

binations, I ſhall ſtate one caſe, in which the ſolid is divided and diſperſed quietly, and one in which its diviſion is accompanied with appearances of violent action. We ſhall afterwards, as we go on, have occaſion to ſee examples of this kind in great abundance. The firſt example is the ſolution of camphor in ſpirit of wine. In this mixture, nothing can be obſerved but the ſlow and gradual diminution of the camphor, and the ſenſible diffuſion of its pungent and hot taſte in every part of the liquor. The ſecond is the ſolution of a bit of marble in muriatic acid, or ſpirit of ſalt. In this experiment, we find that the bit of marble is no ſooner dropped into the liquor, than the particles of it begin to be ſeparated one from another, and to be diſperſed in inviſible atoms through the fluid, ſo that in a ſhort time the marble totally diſappears; and, while this goes on, there is an effervеſcence, or appearance of conſtant agitation, and turbulent commotion, all round the bit of ſtone, and innumerable bubbles ariſe from it, as if the fluid were boiling. The ſame effervеſcence is ſeen here, though leſs violent, as that which appeared in the mixture of the vitriolic acid and volatile alkali.

Beſides theſe ſymptoms of violence, with which certain bodies unite together, it appears, from other particulars, that ſome of them have a very ſtrong propenſity to unite. They will unite under circumſtances very unfavourable, or when the union might be thought to be attended with conſiderable difficulty. Thus, ſome ſubſtances will collect the water that is in the air in the form of inviſible vapour, and will be diſſolved by it, although the air appear very dry, and contain but little of ſuch humidity, ſo that other bodies containing humidity would quickly be dried in ſuch air. The vitriolic acid which we made uſe of is one example of this. There are other ſalts which can be reduced to a dry ſtate, and are uſually kept in that form, but which it is neceſſary to keep in cloſe veſſels, ſhut up from communication with the air; as ſalt of tartar, &c. This phenomenon

is called DELIQUESCENCE, or running PER DELIQUIUM; and the ſalts which have this diſpoſition are called DELIQUESCENT SALTS.

To return again to our experiments. We may take notice, that when a ſolid is thus attacked by a fluid, thus divided and diſperſed through it ſo completely and intimately as to become perfectly fluid along with it, the chemical term for this change of condition is SOLUTION. A ſolid body, to which this has happened, is ſaid to be DISSOLVED, and the fluid which has this effect upon it is called the SOLVENT, or MENSTRUUM. This laſt term is ſaid to have been invented by ſome of the alchemiſts, as the name of ſome very powerful ſolvent, which they made a ſecret, and which required a month to produce its effect.

I wiſh you here to attend carefully to this ſort of combination of ſolids with fluids, and to the characters by which it is diſtinguiſhed, that you may not be miſled by inaccuracies in common language, in which the term ſolution, or diſſolution, is often miſapplied.

It is not uncommon to ſay, "That clay diſſolves, or can be diſſolv-"ed, in water;" becauſe, when put into water, it is firſt ſoftened, and afterwards reduced to a mud ſo fine, that, by ſtirring it well, it can be equally diſperſed through the whole of the water. But, in this caſe, the clay is not intimately *combined* with the water, or reduced along with it to the form of a fluid. The particles of it are only ſeparated from one another, and diſperſed. There is no ſtrong and intimate combination, or change of the clay to the form of a fluid, and incorporation with the water. The mixture has not tranſparency; it is quite muddy, and, if we allow it to ſtand at reſt for ſome time, the whole of the clay ſubſides again to the bottom. The proper name for this kind of mixture, in chemical language, is DIFFUSION. We ſay, that the clay is DIFFUSED through the water.

But, in the caſe of ſolution, the ſolid is incorporated with the fluid, and the mixture is always *tranſparent*. The particles of the ſolid body become inviſible, and, provided we do not afterwards make a

change in the mixture, by adding ſomething elſe to it, or abſtracting ſome of its parts, the ſoluble matter which it contains will never afterwards ſubſide from the fluid, but will remain for ever intimately combined with it.

From theſe examples, you may form ſome idea of the innumerable caſes of union of bodies, with various degrees of the phenomena I have deſcribed and ſhewn you.

But farther: There are ſome other particulars remarkable in the greateſt number of ſuch caſes, which may therefore be alſo enumerated among the more general phenomena of mixture. Thus,

4. We find that, in moſt caſes, we cannot combine ſuch bodies together in every proportion that we might chooſe. There is a limit to the quantity of the one that can be combined with the other. Thus, water will diſſolve only a certain quantity of ſalt: Spirit of wine will diſſolve only a certain quantity of camphor; and in a great number of the caſes, there is even a limit, upon both ſides, to the quantity of each that can be joined with the others, and the bodies can be properly united in one proportion only. This limitation to the quantity of a ſubſtance which can be joined chemically with a certain quantity of another, is denoted, in chemical language, by the term SATURATION. Subſtances which are added to one another, or mixed together exactly, in the largeſt proportion in which they will unite, are ſaid to be mutually SATURATED with one another. Of ſuch ſubſtances we ſhall have examples in the ſalts.

Saturation again is of two kinds. Firſt, ſimple, as in the caſe of common ſalt diſſolved in water. Second, reciprocal, where neither of the bodies can be combined with the other except in one proportion, as in the caſe of a ſaline cryſtal.

5. A diſtinguiſhing mark of chemical union is, that the ſubſtances cannot be ſeparated by filtration, or other mechanical means. It is even difficult, and, in ſome caſes, impoſſible to ſeparate them by the action of heat.

A volatile ſubſtance, when thus united to another which is more fixed, cannot be ſeparated again ſo eaſily by heat applied, as we might be led to expect from our knowledge of their difference in point of volatility. When we apply heat to the compound, the more volatile ingredient is not changed into vapour by that degree of heat which we know to be ſufficient to produce this effect in its ſeparate ſtate. In ſome of the caſes in which the volatile body is combined with a very fixed one, no heat that we can eaſily command is ſtrong enough to change it into vapour. In others, though it may be forced to aſſume the form of vapour, it always requires a heat much ſtronger than that which is ſufficient to produce this change in its ſeparate ſtate; and in many ſuch caſes, when the heat is raiſed ſo high, the volatile body does not riſe alone; the more fixed body is raiſed along with it, and the compound therefore is converted into vapour, without being ſeparated into its conſtituent parts, though the more fixed ingredient would not have been converted into vapour by itſelf in the ſame heat. Several of the experiments which have juſt been exhibited are examples of what we are now ſaying. Thus, in the caſe of water and vitriolic acid, we ſhall be diſappointed if we ſhould expect that by heating this mixture to 212°, the water will quit the acid. In the firſt place, it will not boil till conſiderably hotter, that is, the volatility of the water is diminiſhed. But farther, the water carries off with it a great part of the acid. Again, in the example of volatile alkali, and vitriolic acid; this mixture will not ſeparate at all by heat. It has nothing of that pungent ſmell which indicated the volatility of the ſalt of hartſhorn. And, when we increaſe the heat a good deal, the whole mixture riſes undecompoſed, and the vapour condenſes into a ſolid ſalt. Laſtly, muriatic acid, and crude calcareous earth: This acid has ſuch a great degree of volatility, that we cannot obtain it in a coherent, or a fluid form; and, when pure, it is always a vapour: Yet, when combined with the calcareous earth, it ſtands a red heat, long continued, without appearance of ſeparation.

It is, therefore, impoſſible, in the greateſt number of theſe caſes, to ſeparate from one another, by heat alone, the different ſubſtances which have been thus combined.

6. But, by multiplying our experiments in the way of mixture, a diſcovery has been made, which has been of infinite uſe to us in chemiſtry, and has greatly enlarged our power over a great number of different compounds. The diſcovery I mean is, that the addition of ſome ſuitable third body to a compound of two ingredients, which are united ſtrongly together by chemical combination, will, in many caſes, diſpoſe them to ſeparate from one another. The examples of this are very numerous; but as our purpoſe at preſent is only to form a general idea of theſe facts, I ſhall only call your attention to a few examples, which will be ſufficient to ſerve this purpoſe.

Example 1ſt. If we pour oleum tartari (which is the liquor into which pearl aſhes melt when long expoſed to the air) into a quantity of vitriolic acid, mixed with water, we ſhall have a violent effervescence, and we ſhall ſee a quantity of cryſtalline ſalt fall to the bottom. If we now taſte the liquor, we ſhall find its acidity greatly diminiſhed, if not entirely gone. Thus have we procured a ſeparation of the vitriolic acid from the water.

2*d.* The ſalt which is produced by the mixture of vitriolic acid and the ſalt of hartſhorn, is called vitriolic ammoniac. It has no ſmell. If this be diſſolved in water, and we pour into it an equal quantity of ſoap-maker's ley, called by the chemiſts cauſtic alkali, we ſhall inſtantly be ſtruck by the moſt pungent ſmell of the ſalt of hartſhorn, greatly exceeding the uſual pungency of that ſalt. At the ſame time, a quantity of cryſtalline ſalt will fall to the bottom, without ſmell, and having very little taſte, in compariſon of the vitriolic ammoniac.

3*d.* If, to the ſolution of camphor in ſpirit of wine, which I deſcribed a little ago, we add a quantity of water, we ſhall inſtantly produce ſnow-white clouds in the liquor; and, allowing the mixture to ſettle,

we ſhall find all this white matter collect together at the ſurface, if we have added enough of water, and that it is pure camphor which thus floats at top.

4th. If we pour oleum tartari on the ſolution of marble in the marine acid, we ſhall obſerve a copious ſubſidence of the cloudy matter, which is immediately produced. This falls to the bottom; and, when examined, is found to be the ſame in all its chemical properties with the marble which we diſſolved. This is an evident ſeparation of it from the acid.

This form of ſeparation, when the detached ſubſtance falls to the bottom, is called a PRECIPITATION, and the ſubſtance ſo ſeparated is called the PRECIPITATE.

We have here an opportunity of obſerving further, in all ſuch caſes, that the ſubſtance which is added to the compound, in order to effect a ſeparation of its conſtituent parts, unites ſtrongly with one of the ingredients of the compound, which are thus ſeparated from one another; and we have a new compound.

The four ſeparations juſt now exhibited are, every one, examples of this. Thus, in the firſt experiment, the ſalt at the bottom is found to contain the vitriolic acid united with the ſalt of the oleum tartari; that is, with pearl aſhes. In the ſecond experiment, we find the very ſame ſalt at the bottom. In the third experiment, the liquor is ſpirit of wine, diluted with water, but void of camphor. And in the fourth experiment, the liquor is found to be the ſolution of digeſtive ſalt, which conſiſts of marine acid, and pearl aſhes.

Theſe examples are a ſufficient illuſtration of the new combination which generally reſults from the ſeparation by a third body. There is often a gradation in theſe ſeparations. Thus, ſilver diſſolves in aquafortis, juſt as marble did in that liquor, but after it is diſſolved, it can be ſeparated by mercury. Aquafortis can alſo diſſolve mercury, and the mercury may be ſeparated by copper, lead, or iron. And

copper, diſſolved in the ſame manner, can be ſeparated by iron or lead; and lead, ſo diſſolved, may be ſeparated by iron.

7th. There is one other effect of mixture, which is as general as theſe already enumerated; but the knowledge of it has not yet been found of ſo much importance to the chemiſt, and has therefore been leſs attended to, and is but ſeldom mentioned in chemical books, though it is a remarkable fact, and worthy of more notice. It is this: That in moſt caſes of the union of bodies with one another, if their nature and manner of union be ſuch as to admit of the proper examination, we find that the bulk of the two, when combined, is different from the ſum of their bulks in their ſeparate ſtate. It is generally rather leſs, but in ſome caſes greater. Thus, it is found that 100 cubic inches of water, mixed with 100 of ſpirit of wine, do not make 200 cubic inches of mixture: They rarely make 198, and often conſiderably leſs. Experiments have ſhewn this alſo in mixtures of metals with one another, as in the mixture of mercury and ſilver; and ſtill more remarkably, in the mixture of copper and tin. Although tin is the lighteſt of all the metals, yet this mixture, in a due proportion, produces a mixture which is conſiderably heavier than the copper; that is,—a pound weight of tin being mixed with a pound weight of copper, the mixture will be leſs bulky than two pounds of copper, and far leſs than two pounds of tin. The ſame thing happens in the mixtures of ſalts and water. The difference is commonly not great; but it is ſenſible, and does not proceed from the heat or cold produced. Hence it appears that we cannot form an exact judgment of the proportion of the different ingredients in a compound, by examining its ſpecific gravity, though we know what theſe ingredients are, and what their ſpecific gravities are in a ſeparate ſtate. Therefore Archimedes was in an error when he thought that he had ſolved the problem about the golden crown. I think too that this phenomenon, when attentively conſidered, will be found to be inconſiſtent with ſome common opinions,—as that of the nature of

water, and the cauſe of the difficulty found in compreſſing it. For it is not enough to ſay that the other ingredient occupies leſs room, by lodging in the interſtices between the ſpherules of water, which only touch each other in ſingle points. For, when a certain quantity of ſome ſubſtances are mixed with water, the bulk of the mixture is even leſs than that of the water alone. *(See Hahn de efficaciâ miſturæ in mutandis voluminibus corporum).* The particles of water muſt be nearer than they were before; that is, they were not in contact at the firſt. Mr. Achard of Berlin, and Mr. Briſſon of Paris, have made a vaſt number of experiments on this ſubject.

But this general fact, of the change of denſity in conſequence of the union of bodies into compounds, is not ſo intereſting to the chemiſt as thoſe which I before enumerated.

OF THE THEORIES OF CHEMICAL MIXTURE AND COMBINATION.

In ſtating the theories of the effects of mixture, I may begin by remarking, that during the firſt æra of chemiſtry in Europe, from its firſt introduction to the time of Lord Verulam, the chemiſts had no intelligible theory. This was occaſioned, partly by their being too little acquainted with the principles of reaſoning that are moſt familiar to the reſt of the obſervers of nature, and partly by the nature of their ſcience.

When we give an explication of any extraordinary appearance or property of bodies, we always do this by ſhewing that it is not at bottom ſo extraordinary, or ſo very much unconnected with every thing elſe, but that there is a connection between it and other things, which we know very well, either by a reſemblance which it bears to them in certain particulars, or by its deriving its origin from the ſame cauſes with more familiar events.

But the chemiſts, having confined their attention ſolely to chemiſtry, were ſtrangers, in a great meaſure, to the reſt of the world.

They could not, therefore, explain the chemical facts, by ſhewing a reſemblance between them and other things better known. They were more diſpoſed to explain other things by chemiſtry, than chemiſtry by any thing elſe. Hence aroſe their attempts to ſet up a ſyſtem of medicine, upon chemical principles; and a number of other attempts of a ſimilar kind. And, in their reaſonings concerning the phenomena of chemiſtry itſelf, they never went further than explaining one chemical fact by another. Thus, after obſerving that the ſalts called acids effervеſce with the ſalts called alkalis; if they found any new ſubſtance not an alkali, which effervеſced with acids, they ſaid that it did this becauſe it was of an alkaline nature, or contained hidden alkali in its compoſition.

But, beſide the ignorance of the chemiſts in other things, it muſt be allowed that the nature of their ſcience may have been one cauſe of difficulty to thoſe who would have attempted to improve it in the way of reaſoning and explication. Moſt of the chemical phenomena are ſo extraordinary, and it is ſo difficult to find any thing elſe to which they bear a reſemblance, any thing to which they can be compared, and by which they may be explained, that on this account alone, many of the firſt attempts could not miſs to be unſucceſsful. It is much more eaſy to give a ſatisfactory explication of ſurpriſing facts in ſome other ſciences. When we are told that an ingenious mechanic has contrived to move an immenſe maſs of rock, of enormous ſize and weight, and this by the force of one man only; if we are at all acquainted with the powers of mechaniſm, we ſhall not be greatly ſurpriſed at hearing this. We know the thing to be poſſible, and when we ſee the machine by means of which it was done, we ſhall underſtand his contrivance, and the effect which it produced, completely. We ſhall perceive that his machine is a ſyſtem of levers or wheels, and perhaps of ſcrews, or wedges, or pulleys, properly combined, and that the whole is a contrivance for increaſing force, at the expence of velocity. We ſhall, therefore, underſtand how the

force of one man might be ſufficient to produce the effect; but that he could only move the maſs with a very ſlow, and almoſt imperceptible motion. But, if the ſame rock were moved by a chemiſt, we ſhall be diſappointed if we expect to be as readily ſatisfied, or to underſtand as quickly, the nature of his operation. If we are told that he mixed a ſmall quantity of ſaltpetre, ſulphur, and charcoal together,—laid this mixture under the rock, and applied a ſpark of fire,—and that the rock was inſtantly thrown up with great violence and velocity out of its place; we find nothing here that is reducible to the common laws of mechaniſm—no contrivance for increaſing the force at the expence of the velocity:—we wonder.

Of this uncommon and unaccountable nature are the effects which generally preſent themſelves in chemical experiments. At the ſame time, there is ſo wide a field of inquiry, and it is ſo eaſy for a perſon who applies himſelf to it, to make new diſcoveries of curious and uſeful facts, that thoſe who formerly employed themſelves in improving chemiſtry, took chiefly this way. They made experiments, and enlarged the collection of facts, finding this much eaſier than to explain them. Or, if they attempted explications, it was in the manner already mentioned, and which none elſe underſtood.

Such was the ſtate of our ſcience until the time of Lord Bacon; when it became more commonly known to thoſe who were maſters of other branches of knowledge. From this period, attempts were made to ſpeak more intelligibly than before; by endeavouring to explain the chemical phenomena from mechanical principles. I muſt here obſerve, that as the mechanical powers of bodies are commonly the moſt familiar of any, and moſt diſtinctly underſtood, the mind of man is always prone to have recourſe to theſe for explaining the phenomena of nature. We have always a propenſity to ſuppoſe that any extraordinary effect or power of a body depends on ſome mechaniſm in it, ſomething particular in the form, ſize, arrangement, or motion of its parts. And when any remarkable fact or appearance is ſhewn to de-

pend on principles of this kind, it is generally thought to be well explained. The account thus given of it is plain and intelligible.

Lord Bacon, therefore, recommended this ſort of reaſoning, in preference to the language which was common in his time among the chemiſts; and Mr. Boyle afterwards attempted to introduce and ſupport it more fully. But the conſequence was, that for ſome time after, this ſort of reaſoning was applied to chemical phenomena with ſo much boldneſs, and ſo little judgment or conſideration, as quite diſguſted thoſe who had real diſcernment; and they formed an opinion, that chemical facts could not be explained in a ſatisfactory manner by reaſoning from mechanical principles.

There is a noted example of a bold reaſoner in this way, in a French author, Lemery, who in other reſpects was a chemiſt of great merit; but he was led into this manner of reaſoning by the faſhion of the times. According to this author, a fluid which has the power of diſſolving a ſolid body, abounds with ſharp and pointed particles, having the forms of needles or wedges, which are agitated in the fluid with a rapid and confuſed inteſtine motion. The ſolid again has pores of ſuch ſizes and ſhapes, as are fitted to the pointed particles of the fluid, in conſequence of which they are penetrated and torn aſunder. In like manner, he explained the precipitation by a third body, as when potaſh cauſes marble to be precipitated from its ſolution in aquafortis, by ſaying that the particles of the potaſh were porous and ſpongy, and, by this configuration and confuſed motion, they took hold of, and broke the ſpiculæ of the acid, &c. &c. This ſuppoſition appeared at firſt to explain ſome particular facts very conveniently. Thus aquafortis diſſolves iron or copper, but not gold; the reaſon was plain, becauſe iron and copper have wider and more numerous pores than gold. But aquaregia diſſolves gold, and does not act on ſilver. In like manner, mercury penetrates and diſſolves gold, and will not touch iron, and very difficultly copper. Many other examples of the ſame kind might be adduced. Indeed, this ſort of

theory, in the way in which it was managed, was quite inconſiſtent with ſound mechanical principles, upon which alone, however, it was meant to be founded. The motion of the particles of fluids, neceſſary for producing the agitation, could not continue; they muſt neceſſarily ſtop one another. Yet ſpirit of ſalt, though kept for ages, will act on marble; and we have proof by experiment, that it is not neceſſary that the particles of a body be fluid or moving, to make it capable of acting upon another. There are ſome examples of ſolid bodies acting on one another, and mutually diſſolving, and we cannot imagine that the particles of ſolids are moving among themſelves.

The experiment which I have often mentioned to you, in which an intenſe cold was produced by mixing dry ſalt and dry ſnow, is of this kind: They melt into a brine, that is, they diſſolve one another. In like manner, corroſive ſublimate, and regulus of antimony, lime, and cryſtallized mild volatile alkali, act on each other, and unite in a different manner from their former ſtate, exhibiting a true chemical ſolution.

It is needleſs to inſiſt longer on theſe whimſical theories, ſo gratuitous and unſupported by evidence, and ſo deficient even in the plauſibility of explanation. We can have no diſtinct conception of ſuch action, and without this no theory can have any value.

Theſe, and ſome other attempts of the ſame kind, to explain the chemical phenomena, appeared therefore quite crude and unſatisfactory to thoſe who had more diſcernment and penetration than ordinary. But we had no chemical theory that connected this ſcience properly with other parts of our knowledge of nature, until Sir Iſaac Newton publiſhed that edition of his Optics, at the end of which he has given a number of Queries, containing his ſpeculations and conjectures concerning many of the curious and difficult facts which occur in the ſtudy of nature. There are a number of thoſe queries that relate to chemiſtry. In theſe he lays open a view of the more remarkable phenomena which occur in chemical mixtures, which is altogether his own, and which is much more ſatisfactory, and makes them

appear much more conformable to the reſt of nature, than any that had ever been offered before.

This great author had already given to the world his much admired ſyſtem of the univerſe, in which he explained the connection of the heavenly bodies with one another, and the regulation of their motions, by ſhewing that they are retained in their orbits by the ſame power, or principle of motion which determines a ſtone to fall to the ground; a power commonly called *Gravitation*, or the *attraction of Gravitation*. He conceived that there are other forces or principles of motion in nature, more or leſs ſimilar to this, by which certain bodies act, or appear to act, at a ſenſible diſtance, on one another. This is evident in the caſe of the magnet and iron, and in electrified bodies. He ſuſpected that there were ſtill other ſimilar forces, whoſe ſphere of action was ſtill ſmaller, even ſo ſmall as to eſcape our ſenſes. He imagined that theſe are manifeſt in the attraction and coheſion of poliſhed planes, and of metals, in the attraction of ſmall vacuities for different fluids, commonly called *Capillary Attraction*, and in the inflection and deflection of light, as it paſſes near the edges of ſolid bodies. *It is worthy of particular remark*, that the force which acts in theſe laſt inſtances is amazingly great. Notwithſtanding the aſtoniſhing velocity of light, about 200,000 miles in a ſecond, and the almoſt unmeaſurably minute ſpace through which the deflecting force acts on the light, it turns it greatly out of its former direction. From the quantity of this deflection, we could calculate the proportion of this force to that of gravity: For Newton, with wonderful ſagacity, has been able to give us the exact meaſure of the diſtance at which this deflection is performed. This force is not leſs than twenty millions of millions of times greater than gravity *.

* Dr. Herſchell's diſcovery of the refrangibility of heat, in preciſely the ſame manner and degree as light, ſhews the immenſe force which connects it with other matter; a force fully adequate to the moſt remarkable effects which we ſee it produce.

EDITOR.

Sir Iſaac, reflecting on theſe examples of the action of bodies on one another, and being ſtruck with the effects he had obſerved in making chemical mixtures, ſuſpected that theſe laſt might depend on powers, or principles of motion, ſomewhat reſembling the others.

You cannot have a more ſtriking view of his ideas on theſe ſubjects, by any account of them, than that which you will get by reading thoſe paſſages at the end of his Optics, (Queſt. 22, &c.) in which he firſt communicated theſe conjectures of his to the world. His ſtyle is ſo ſimple, and at the ſame time ſo nervous, that it cannot fail to make an impreſſion.

By theſe Queries, in which Sir Iſaac Newton firſt propoſed the doctrine of attraction in chemiſtry, an intelligent chemiſt cannot miſs to perceive that he had acquired extenſive and accurate knowledge in the different parts of this ſcience: And, when we conſider the time when they were publiſhed, and how little had been done to explain the chemical phenomena in a ſatisfactory manner, we ſee in theſe queries a proof of that capacity of mind, and that acuteneſs and originality of genius, by which Sir Iſaac diſtinguiſhed himſelf on every occaſion. Not only is the path pointed out, in which great reputation has been acquired by ſome of the moſt eminent chemiſts, but the chief ſteps are actually made, and the manner in which farther progreſs may be made, is diſtinctly deſcribed.

His explanation of the chemical phenomena was received, by the unprejudiſed, with the higheſt admiration and pleaſure: And there is no doubt but that it opened a view of ſome principles of action in nature, which certainly exiſt, and which, with ſome few modifications, explain many of the moſt remarkable chemical facts. It is evident that there are ſuch *principles* of motion in nature, as Sir Iſaac pointed out; for there is no denying that there are ſuch *motions*. The reality of the powers which connect the particles of matter is clearly proved by a multitude of facts and experiments: And, that

attractions ſomewhat reſembling theſe, act in chemical mixtures, and that with great force, is equally plain from the phenomena.

For, when marble, for example, or iron, are diſſolved in the muriatic acid with rapidity, and appearances of violence, a very great force muſt act to produce this effect, overcoming the ſtrong coheſion of the particles of thoſe bodies. Were we to attempt to divide them mechanically into very minute parts, it would coſt us as much labour and toil to effect this; and our utmoſt exertions, though ever ſo long continued, would never divide them into atoms ſo minute as thoſe into which they are divided by chemical ſolvents. And, even after they are divided by theſe, we have the plaineſt evidence of the continued action of an attractive force, which prevents the heavy matter of the ſolid from being ever depoſited again by the leſs ponderous fluid, and which binds to the particles of the iron or marble, the particles of the volatile ſolvent, thereby greatly diminiſhing, or repreſſing, the volatility which it had in its ſeparate ſtate.

Sir Iſaac's theory, therefore, explains all the moſt difficult parts of the ſubject. It ſupplies a force by which ſolid bodies are divided, and remain for ever ſuſpended. It explains why ſome ſalts are deliqueſcent, and why volatile and fixed ſubſtances, when united in this way, are afterwards ſo ſtrongly connected together, and ſo difficultly ſeparated from one another by heat.

I muſt not, however, omit to obſerve, that ſome of the effects which were at firſt imputed to the violent action of theſe forces, have been ſince diſcovered to proceed from other cauſes. Such is the efferveſcence which occurs in making many chemical mixtures. Sir Iſaac ſuppoſed it to proceed from the violent friction and colliſion of the particles of the different bodies, while they ruſhed together to unite. It was very natural for him to form this ſuppoſition. It was even, I may ſay, a beautiful part of his theory, as it preſented to the imagination, an explanation which appeared at that time very probable, and was eaſily underſtood. *(See Note 15. at the end of the*

Volume.) It is known however now, by late experiments and diſcoveries, to proceed, in moſt caſes, from the liberation of ſome elaſtic aerial matter, which was contained in thoſe bodies in a condenſed ſtate. And the heat is now explained on other principles, viz. by a diminution of capacity in the compound, to contain the heat which was contained in its ingredients, or (to expreſs it more cautiouſly) by the liberation of a quantity of heat not neceſſary for that temperature of the compound. But all the other phenomena of mixture are ſtill conſidered as depending entirely on theſe ſpecific attractions.

Notwithſtanding the very great merit of this theory in compariſon with others, it was not very well received at firſt by the chemiſts abroad. They objected to the word attraction, as implying either an active quality in matter, which we cannot conceive to be poſſeſſed of activity, or ſome connecting intermediate ſubſtance, by which the particles of bodies were drawn together, and for the exiſtence of which no proof is offered. They therefore choſe to ſubſtitute the term AFFINITY inſtead of ATTRACTION.

But their objections to the word *attraction* were certainly unreaſonable. Sir Iſaac, in the beginning of theſe queries, expreſsly declares, that when he uſes this word, he does not pretend to aſſign the cauſes, natures, or manners of acting, of thoſe forces by which bodies are diſpoſed to ruſh together into union. He only means the forces themſelves, or the diſpoſition to join, manifeſted by the fact itſelf, leaving it to others to diſcover the cauſe, and obſerving that it is improper to attempt it, until we become better acquainted with its manner of acting, by ſtudying the facts. And when the word is uſed with this precaution, merely as a term expreſſing a fact, a phenomenon, I do not ſee any advantage in preferring *affinity* to it. It would ſound very ill, to ſpeak of the affinity of gravitation, of electricity, or of magnetiſm. Attraction is more expreſſive; and affinity implies, or ſuggeſts, ſome ſimilarity, which, in moſt caſes, is not agreeable to fact, ſeeing that we generally obſerve the greateſt diſſimilarity in thoſe bodies

which are eminently prone to unite. *(See Note* 16. *at the end of the Volume.)*

As Sir Iſaac's theory, however, is now well eſtabliſhed wherever the ſcience of chemiſtry has made any progreſs, and, as we ſhall find it moſt extenſively uſeful to us in aſſiſting us to underſtand a great variety of the chemical phenomena, I ſhall conſider it with more attention, and endeavour to give you as clear and correct an idea of it as poſſible, as well as of the facts which it explains. With this intention, I ſhall make a few remarks on it.

1. The firſt I ſhall offer is, that it differs from ſome other kinds of attraction in this reſpect, that, ſo far as we can perceive, it cannot ſet in motion large and viſible maſſes of matter. It only acts in ſuch a manner, as to diſpoſe the minute and inviſible particles of one ſubſtance, to ſeparate from one another, and unite themſelves ſeverally to the minute and inviſible particles of another ſubſtance. An obvious example of which we have in a lump of ſalt or ſugar, and a quantity of water.

It is a neceſſary conſequence, therefore, of the *nature* of theſe attractions, that the attraction of *coheſion* is an antagoniſt to them, or counteracts them more or leſs in all caſes. By the *attraction of coheſion*, or the *coheſive attraction*, is meant that attraction which ſimilar particles, or the particles of the ſame kind of matter, manifeſt for one another. *(See Note* 17. *at the end of the Volume.)*

We have an obvious example of the action of this ſort of attraction in two bits of lead, or of elaſtic gum, which, being made quite clean, and cloſely applied to one another, will join and cohere as ſtrongly as if they had been melted or ſoldered together, in thoſe parts of their ſurface that have been brought into very cloſe contact. Iron affords another example; different pieces of it uniting perfectly together, when they are ſoftened by a ſtrong heat, and very cloſely applied. Clay is an example ſtill more familiar, and, in my opinion, perfectly ſimilar to all the other examples. Although, in theſe caſes, we do not perceive

the action of cohesive attraction, in making the masses cohere, except when they are brought into what appears to our senses the closest contact; yet other experiments have shewn that it can act at small distances, only that its power is diminished so fast as the distance is increased, that it soon becomes imperceptible. The experiment that I have chiefly in view here, is that first made by Mr. Huyghens, and mentioned in the Phil. Trans. No. 86. He found that two finely polished plates of glass attracted each other, and that the one lifted the other, even when a single fibre of silk was interposed, but not when two fibres crossed one another.

Without this cohesive attraction, no body would have solidity or hardness, but would be a powder, or a liquid, or in the form of vapour, if heat were present, or in the state of inconceivably subtile unconnected particles. This attraction is in general strongest in the most solid, hard, and dense bodies; less strong in those which are soft or brittle; particularly modified in those which are tough and ductile. Its effects are reduced to a great degree of weakness in those which are fluid, and almost to nothing in those which are in the form of vapour *. It is in all these cases counteracted, less or more, by the repulsive force of the matter of heat.

This sort of attraction (viz. cohesive), as you will perceive, does not in the least counteract some other kinds of attraction, by which certain bodies act on one another, such as the attraction of the magnet and iron, and the attractions of electricity and of gravitation. But the chemical attractions are directly counteracted and opposed by it. When a lump of iron, for example, is thrown into nitrous or muriatic

* The immediate, or what we may call the *formal* cause, of these different modes of aggregation, are distinctly, but concisely, explained in Newton's questions, at the end of his optics. They are considered at great length by Mr. Boscovich, in his Theory of Natural Philosophy, a work of the greatest ingenuity, and richly deserving the *serious* perusal of a philosophical chemist.

EDITOR.

acid, there is a powerful chemical attraction, which diſpoſes the particles of the iron, and thoſe of the acid, to unite together. But it is evident that the coheſion of the parts of the iron with one another is in oppoſition to their union with thoſe of the acid; and were not their attraction for the acid particles ſtronger than their coheſion with one another, the iron could not be diſſolved. It is ſtronger, however, and the conſequence is the diſſolution of the iron, notwithſtanding the ſtrong coheſion of its particles. *(See Note* 18. *at the end of the Volume.)* But although this coheſion is not ſtrong enough to preſerve the iron from being diſſolved, it muſt neceſſarily reſiſt or counteract, to a certain degree, the powerful attraction of the acid; and the iron would be diſſolved with much greater rapidity, were it in our power to deprive it of its coheſive attraction before it is put into the acid.

There are even examples, in which, by increaſing the power and action of coheſive attraction, we can enable it to overcome a chemical attraction, which it was not able to overcome before. In the hammering of iron, and other metals, the latent heat is forced out, the chemical attraction between it and the metal being overcome, by increaſing the power of the coheſive attraction, in conſequence of the condenſation of the parts of the metal, which the hammering produces *. The metal, after being hammered, is both denſer and harder than before. And the coheſive attraction, being in all caſes an antagoniſt to the chemical attraction, by which latent heat is retained in bodies, when the one of theſe attractions is increaſed, the other muſt give way, or be overcome, more or leſs. Even the chemical attraction of acids for iron is counteracted, and reduced in its power and efficacy, by hardening the iron.

In confirmation of theſe remarks, I obſerve that we have learned, by the moſt extenſive experience in chemiſtry, that bodies which have

* See what was delivered on this ſubject in page 140. The diminution of the chemical attraction, by hardening the iron, is very doubtful.

EDITOR.

chemical attraction for one another, act and unite together much more readily, when we take them in a ſtate in which their coheſion is weak, than when it is ſtrong. Of this you will ſee numerous examples as we proceed. We have a good example of this counteraction of coheſive and chemical attraction in the phoſphorates of ſilver and of nicolum. Theſe compounds retain the phoſphorus while they are fluid by heat, but when the coheſive attraction increaſes by the diminution of the heat, the phoſphorus is thrown out, and burns on the ſurface of the metals, while they congeal (*Annales de Chymie*, T. 13.)

And this proceeds from that peculiarity which I mentioned juſt now in the nature of chemical attractions, that they do not act ſo as to diſpoſe large, or any way perceptible maſſes of matter, to move towards one another, and join together in their maſſy ſtate. They only act ſo as to diſpoſe the minute and inviſible atoms of one ſpecies of matter to unite ſeverally with the minute and inviſible atoms of another, or of ſome others. And theſe attractive forces between the atoms of ſuch different kinds of matter, act ſo powerfully as to tear aſunder and totally deſtroy the maſſes which theſe different kinds of matter formed ſeparately before.

2. We may in the next place remark, with regard to this attraction, that when two bodies unite in this manner, they are each of them divided into particles of ſuch extreme minuteneſs, that the utmoſt efforts of imagination cannot form an idea of it. I had one day a deſire to ſee into how many ſenſible parts a body may be actually divided by ſolution. I found that one grain of ſilver gave a very ſenſible tinge to about 38 Troy ounces of water, or 18060 grains. I found by repeated trials, that a drop of water weighs very nearly a grain, for 50 drops weighed from 49 to 55 grains. An ordinary microſcope exhibits 100,000 ſenſible parts in a drop when ſpread out. Suppoſe that it exhibits only 1000 ſuch parts: In every one of theſe there are many particles of the ſilver, for the viſible particles are all little flocculi, of different ragged forms, and therefore conſiſting of many atoms. From

this obſervation, I was well entitled to ſay that a grain of ſilver could be divided into much more than 18 million of ſenſible parts. You may form ſome notion of this number by this circumſtance, that this number of common peaſe, laid cloſe to each other, would occupy 81,43 miles. This is not a ſingular or extraordinary example; we have reaſon to think that a ſimilar diviſion takes place in all caſes.

3. I obſerve that the influence or energy of chemical attraction reaches only to an exceedingly ſmall diſtance round the particles of bodies, a diſtance ſo extremely ſmall, that it is not perceptible; and, if we truſt our ſenſes, no action ſeems to take place until the particles come into the cloſeſt contact.

By attending to theſe particulars in the nature of chemical attraction, we are enabled to underſtand the propriety of ſome canons or rules relating to the practice of chemiſtry, or to the moſt proper manner of performing many chemical operations, or of making experiments.

Firſt Rule. When it is deſired to make two bodies act chemically on one another, to produce any change, or to obtain any product in chemiſtry, it is in moſt caſes neceſſary that one or both be fluid when they are mixed, or that they be rendered fluid, or diſpoſed to fluidity, immediately after. The caſes are very few indeed, where any conſiderable action can be induced without previous fluidity. You will get a very clear conviction of its utility, by taking a ſmall quantity of ſal ammoniac, and of ſalt of tartar in dry powder, and mixing them. No ſenſible change is produced; but pour in a little water, and inſtantly you have a ſtrong ſmell of ſpirits of hartſhorn. This is inſtantly formed by the aſſiſtance of the water.

In like manner, dry acid of tartar, and dry ſalt of tartar, remain inactive when mixed; but a little water being poured in, you have the violent action of the ingredients, and obtain a compound ſalt.

The dry ſalt compoſed of copper and aquafortis, being wrapped up in tinfoil, alſo remains inactive; but if, before wrapping up the

ſalt, you very ſlightly wet the tinfoil with a pencil dipped in water, and quickly roll it up after ſprinkling the ſalt on it, you have a moſt violent action, accompanied with exploſions, and even a bright flame and burning heat. The tin leaf is all corroded or eaten into holes.

The propriety of this rule ariſes from many conſiderations. In the firſt place, the fluidity produces an extent of contact unattainable in any other way. The fineſt powders touch each other but in few points, and the greateſt part of each particle of the powder lies from another far beyond the reach of chemical attraction. But the very atoms, when floating in the fluid, are applied to the other ſubſtance all round, at a diſtance fully within the ſphere of action.

But, ſecondly, it appears that ſome chemical actions evidently aſſiſt one another, not perhaps directly, but by counteracting the coheſive attraction in the particles of ſolid matter. The coheſive attraction is unqueſtionably an antagoniſt to all chemical actions whatever; it *muſt* be overcome in the firſt inſtance. This is one of thoſe chemical facts or laws, which the chemiſts on the continent call examples of *prediſpoſing* affinity. Some curious examples will occur afterwards. Perhaps we may not underſtand the cauſe or nature of this fitneſs or prediſpoſition in every caſe; but we may uſe the term as the expreſſion of a phenomenon, an undoubted and important fact,—important, as it very often enables us to accompliſh what would otherwiſe be attempted in vain.

For ſimilar reaſons, it is not unfrequently the practice to convert ſubſtances into vapour, in order to make them unite. Here the coheſive attraction is completely deſtroyed. The vapours are moſt intimately mixed, and they unite when condenſed. There are ſeveral inſtances, however, where two gaſes, that is, permanently elaſtic fluids, ſhew no diſpoſition to unite when completely formed, yet will unite, if one of them be preſented to the other in its naſcent ſtate, that is, in the very act of formation. This happens in the caſe of inflammable and vital airs.

Second Rule. That the ſolution of ſolids in fluids is promoted by previouſly dividing the ſolid mechanically, or otherwiſe enlarging its ſurface, as by pulveriſation, granulation, filing, plating. The reaſons for this are too plain to require illuſtration. Theſe preparations increaſe the ſurface of action.

Third Rule. Frequent agitation of the ſolvent alſo promotes ſolution, by removing the ſaturated parts of it. This is a curious ſubject, and of moſt extenſive influence. *(See Note* 19. *at the end of the Volume.)*

A Fourth Rule is, That the action of bodies in general upon one another, and their union together, is promoted by moderate heat. This rule is founded upon a very extenſive experience. I do not remember any exception from it, any example of bodies diſpoſed to act on one another, in which the action is not promoted and increaſed by a moderate increaſe of their heat. Thus ſalts and water act in general more powerfully as the mixture is hotter There are even examples by which we might be led to conclude that chemical activity would not exiſt without more or leſs heat. When we diminiſh heat, the chemical action of bodies becomes languid; and in ſome few caſes, when it is greatly diminiſhed, chemical action ceaſes entirely. This is the reaſon why we cannot produce a greater cold with ſalt and ſnow; and it is plain that the power of nitrous acid and ſnow is limited in the ſame manner, as in a very intenſe cold the acid ſhews a diminution of its attraction for the water by cryſtallizing. Query, Is this occaſioned by the increaſed action of coheſive attraction, in conſequence of the diminution of heat, which is an antagoniſt to it? Does a gentle heat promote chemical action and union by weakening the coheſion of the aggregate, while a ſtrong heat weakens, in many caſes, even chemical union, by introducing a repellency of the parts of the compound, of different volatility? *

* The experiments of Mr. Berthollet, and of Mr. Seguin, ſhew ſome very remarkable changes, and alternations in the affinities of bodies, by a gradual increaſe of their heat. They may be explained by ſuppoſing that the attractions of different

As the general rule, that the chemical action of bodies, and their union with one another, is promoted by moderate heat, has been long eſtabliſhed upon the moſt extenſive experience, it has given occaſion to ſome operations, which are often performed to promote the chemical action of bodies upon one another, ſuch as, DIGESTION, CIRCULATION, and COHOBATION.

OF ELECTIVE ATTRACTIONS.

The remarks which have been now made are equally applicable to all caſes of chemical action, and the union of bodies with one another. But we muſt now give a little more attention to thoſe numerous caſes, in which a third body frequently acts on a compound of two ingredients, ſo as to ſeparate theſe from one another, and join itſelf to one of the two. An attention to theſe caſes is of the greateſt importance in chemiſtry; for it is by our knowledge of them that we are enabled to obtain the decompoſition of many compounds, the ingredients of which cohere too ſtrongly to be ſeparated by heat, or by any other means. Indeed, I may almoſt ſay, that a complete knowledge of the general laws of this phenomenon is nearly the whole of chemical ſcience.

The caſes in which this method may be employed are very numerous. There are numerous examples in chemiſtry, in which the ſame body has a chemical attraction for each of a number of others, or a diſpoſition to unite with them. But ſuch is the nature of that power by which theſe unions are produced, that the body is capable of being firmly joined to one only of theſe others at the ſame time. Whenever two bodies are joined by chemical attraction, a third can ſeldom, if ever, be admitted into the compound. The diſpoſition of theſe two to unite with others, is diminiſhed, and, ſo far as we can perceive, it is diminiſhed the more, in proportion to the force with

bodies vary in different proportions, by the ſame variation of temperature; ſo that in one temperature they are in equilibrio, and inactive, but not in others. EDITOR.

which theſe two are joined together. If this force be ſtrong, the attraction of other ſubſtances is rendered ineffectual; or, if it have any effect, which I think it has in many caſes in which this is not ſuſpected, it is only to weaken a little the force with which the two bodies are joined, but not ſo much as to ſeparate them. But if, on the other hand, the force with which two ſuch ſubſtances are united, is weak, and we add a third, which has a ſtrong attraction for one of the two, it immediately unites itſelf to that one, and ſeparates the other, or, the other is let go, and ſeparates. Thus we form a new compound; and this new compound can, in many caſes, be decompoſed in its turn, by another body, in the ſame manner. Examples of this are more eſpecially called ELECTIVE ATTRACTIONS, or EXCHANGES. *(See Note 20. at the end of the Volume.)*

Sir Iſaac Newton has not ſtated expreſsly any other ſuppoſition to explain this phenomenon, than that the added matter has a ſtronger attraction for one of the ingredients of the compound, than theſe have for one another. And no doubt this is one neceſſary cauſe; and accordingly, we generally find it more difficult to decompoſe by heat, the new compound thus produced, than the former one, ſuppoſing the volatility of the volatile ingredient, and the fixedneſs of the fixed one, to be the ſame in both caſes. But ſtill this ſuppoſition alone, of a ſtronger attraction, ſeems hardly ſufficient; for why ſhould not the more ſtrongly attracted body unite itſelf to that ingredient which attracts it, without diſlodging the other, and thus produce a compound of three ingredients, two of which, though without attraction for one another, might, however, be combined by their attraction for the third? As this happens but very rarely, there is reaſon to ſuſpect that there is a cauſe of the decompoſition of the firſt compound, and this is poſſibly a repulſion between the added body and that ingredient which it ſeparates and throws looſe, while it unites with the other. There are many phenomena in chemiſtry that appear to ſuggeſt ſuch a ſuppoſition, and Sir Iſaac himſelf throws out a hint to this purpoſe, in another part of his Queries. But we

ſhall have better opportunities hereafter, to point out ſome caſes in which ſuch repulſions appear to take place. *(See Note 21. at the end of the Volume.)*

It was ſaid juſt now, that the knowledge of the different caſes of elective exchanges enables the chemiſt to reſolve into their principles a great number of compounds, which it would be impoſſible to decompoſe by any other method, and that they are, therefore, the foundations of a great number of curious and uſeful operations.

The celebrated Mr. Geoffroy of the French Academy, perceiving what advantage it would give in chemiſtry to have a diſtinct and comprehenſive view of all the caſes in which ſuch exchanges may happen, firſt thought of reducing them into the form of a table, which preſents them to view at once, and he called it the TABLE OF ELECTIVE ATTRACTIONS, or of *Exchanges*, or of *Affinities*. It is not proper at preſent to ſhew and explain this table, becauſe you are not acquainted with the facts to which it refers. But to give you an idea of the manner in which it is conſtructed, obſerve, that it has been found by experience, that a number of different metals can be diſſolved in aquafortis, in the ſame manner as ſalt is in water. It has likewiſe been diſcovered that theſe, by their different degrees of attraction for aquafortis, are capable of ſeparating one another, in a certain determinate order, by elective attraction. Mr. Geoffroy, in order to recal theſe facts to the mind, conſtructed one of the columns of his table in this manner: He placed at the head of it nitric acid, or aquafortis, and under it the different metals which may be diſſolved in that acid, placing iron uppermoſt, next under it copper, under copper lead, under lead quickſilver, and under that ſilver: Thus,

Nitric Acid
Iron
Copper
Lead
Quickſilver
Silver.

This arrangement is equivalent to ſaying at full length, that if, to a ſolution of ſilver in aquafortis, we add a little mercury, the ſilver will be ſeparated from the acid, and we ſhall have a ſolution or compound of aquafortis and mercury; if we add to this ſolution a bit of lead, the mercury will fall to the bottom, and the liquor will be a ſolution of lead in aquafortis. In like manner, copper will ſeparate the lead, and iron will ſeparate the copper. Alſo, iron will ſeparate lead, or mercury, or ſilver; and lead will ſeparate ſilver. In ſhort, any of the inferior metals will be ſeparated by any of thoſe placed above it. Such a column repreſents a train of chemical operations.

This was a very ingenious thought of Mr. Geoffroy, and his table was received with general applauſe, and has been exceedingly uſeful to the chemiſts. It was conſtructed with that ſkill and accuracy, which might have been expected from his extenſive knowledge and great experience in chemiſtry. But, being the firſt attempt of this kind, it could not be ſuppoſed to be entirely perfect; and, accordingly, ſucceeding chemiſts have made a number of objections to the conſtruction of different parts of it. With reſpect to thoſe objections, I ſhall only remark at preſent, that theſe attractions are capable of being affected by ſome circumſtances, which, if they had been attended to, would have given chemiſts more diſtinct ideas on the ſubject, and might have prevented ſome of thoſe diſputes. One example of this is in the power of heat, which, when varied in its ſtrength or intenſity, produces a conſiderable difference in the force wherewith certain bodies attract one another, or remain combined. This happens remarkably, when one of theſe bodies is fixed, and the other volatile. If both have the ſame degree of volatility, increaſe of heat will not produce (ſo far as we know) any variation of their attraction, unleſs it be perhaps to increaſe it. But when the one is volatile, and the other fixed, variation of heat produces a conſiderable difference in the force of attraction. There are degrees, or intenſities of heat, which will weaken the attraction and connection of two ſuch bodies, and, in

ſome caſes, will actually make them ſeparate from one another; and, therefore, muſt make them very liable to be ſeparated by the attraction of other bodies, which would not otherwiſe have that power. This is a fact, of which you ſhall be fully ſatisfied hereafter, when we enter into a particular account of the objects of chemiſtry *.

There is another circumſtance which occaſions ſome variations, or apparent variations, of the elective attractions; variations, which, when they were firſt diſcovered, were ſo unexpected, that they appeared to overthrow the whole ſyſtem. The circumſtance I mean is the *quantity* of an attracting ſubſtance, that is applied to another, or to a compound †. When the quantity of ſuch attracting ſubſtance is great, its power over the other, or the amount and effect of its attraction, is thereby increaſed; the large quantity of the ſubſtance acting at the ſame time, having thereby the more power. This, in ſome caſes, enables a more weakly attracting ſubſtance to overcome the attraction of a ſtronger, when the more weakly attracting ſubſtance is uſed in much greater quantity than the ſtronger. There are many examples of this in chemiſtry. And it appears to be for this reaſon that an abundance of the matter of heat, or caloric, overcomes more or leſs the coheſive attraction of bodies, and either expands them, or unites itſelf with them in the form of latent heat, and then melts them

* There are other caſes, leſs remarkable, but equally conſtant, where it appears that the effect of heat in weakening the attractions of bodies, does not vary in the ſame degree, in different ſubſtances, which do not ſenſibly differ in volatility. And there are a few caſes, where intermediate compoſitions obtain, in complex ſubjects, in certain temperatures, by which the mutual actions of the ingredients are often changed. This frequently happens in watery ſolutions.

EDITOR.

† See Decompoſ. par l'Acide Marin de pluſieurs ſels vitrioliques et nitreux, par M. Cornette. Mem. de l'Acad. 1 79.

See alſo the later publications of Mr. Berthollet, on Affinities, in which he has conſidered this circumſtance with great diſcernment.

EDITOR

or converts them into vapour. *(See Note 22. at the end of the Volume.)*

Upon the whole, Mr. Geoffroy's table, conſidered as the firſt thought, and firſt attempt in this way, is highly worthy of praiſe. But ſuch a multitude of diſcoveries in chemiſtry have been made ſince his time, that his original table gives now but a narrow and imperfect view of the numerous facts which a table of this kind can be made to repreſent and ſuggeſt. Accordingly, ſeveral improved and more comprehenſive tables of elective attractions and exchanges have been drawn up by different chemiſts; but none has employed ſo much pains on this ſubject, or given ſuch a comprehenſive and copious table, as the late Profeſſor Bergmann of Sweden, whoſe works on this and other parts of chemiſtry are tranſlated into our language. And ſince that time, Mr. Morveau in the New Encyclopædia. *(See Note 23. at the end of the Volume.)*

Among the moſt remarkable additions which have been made to theſe tables, ſince Mr. Geoffroy's firſt attempt, are the caſes of DOUBLE ELECTIVE ATTRACTIONS. Theſe had been but little attended to, or underſtood in his time, although they happen in many important operations in chemiſtry, and the knowledge of them is indiſpenſably neceſſary on many occaſions.

By a double elective attraction, or double exchange, is meant, the action of two compounds on one another, each of them conſiſting of two ingredients, and each of thoſe ingredients having an attraction for one of the ingredients of the other compound. The conſequence of applying two ſuch compounds to one another is, in many caſes, that they are both decompounded, and two new compounds produced in their ſtead; each of the ingredients of the one joining itſelf to that ingredient of the other compound for which it has an attraction.

As the knowledge of theſe caſes is very uſeful, I began in the year 1756 or 1757, to repreſent them by diagrams, and to ſubjoin theſe to the table of elective attractions, which I gave to the gentlemen who

attended my lectures; and ſome other chemiſts began ſoon after to do the ſame thing, with perhaps a different mode of expreſſion. When I explain the table, I ſhall ſhew you ſome of the ways in which they may be repreſented. I cannot juſt now enter more particularly into this ſubject, as the different ſubſtances which have attractions for one another are not yet known to you.

Before I conclude this part of our courſe, it will be proper to explain to you ſome terms of diſtinction which will frequently occur in the courſe of your reading, eſpecially of the foreign writers.

The union of *aggregation* is that where a number of particles of the ſame kind are retained in one maſs, called the *aggregate*, while the particles of which it conſiſts are called its *integrant* parts, ſeeing that their ſum conſtitutes the aggregate, the integer, or whole. Theſe terms expreſs very well the mere relation of co-exiſtence, but I think that the term attraction of aggregation ill expreſſes the bond of union. It does not characteriſe it ſo well as the common term of *coheſion*, which I ſhould therefore prefer. It is thought proper, however, to diſtinguiſh this bond of union of particles of one kind from that which eſſentially connects particles of different kinds, and is more peculiarly chemical. The attraction of aggregation or coheſion may be overcome by mere mechanical force, whereas the other requires elective or chemical attractions.

The attraction of *compoſition* expreſſes very preciſely and perſpicuouſly the force which unites particles of different kinds, compoſing of them, ſo united, particles of a kind different from both. The attraction of compoſition unites a particle of vitriolic acid with a particle of foſſil alkali, and theſe two united compoſe a particle of Glauber's ſalt. Each of the component particles is called an *ingredient*, and the united particles form a particle of the *compound*. Particles of this kind, or ingredients, cannot be ſeparated by mechanical force, but require the action of heat, or of a third kind of ingredient, to effect a ſeparation.

Of this kind of union there are different claſſes, which chemiſts have attempted to diſtinguiſh and mark by different names.

When different metals are mixed together, we obtain particles which retain the former properties of the ingredients. In like manner, the mixture of different liquors is frequently attended by no change of chemical properties. The ſame thing obtains in the ſolution of ſalts in water, of reſins in alcohol, of ſulphur in oil, &c. Yet is the combination chemical, and cannot be undone by mechanical forces. Such combinations are uſually called *mixtures*.

In other caſes, the properties or ſenſible appearance and chemical action of the ingredients are totally changed, and new ones induced. Thus, aquafortis, and ſoap ley, are two of the moſt acrid and corroſive ſubſtances in nature; their taſte is intolerable, and they quickly deſtroy our organs; but, united, they compoſe ſaltpetre, a mild and harmleſs ſalt.

This is true chemical *combination*, or *compoſition*; and the ſubſtance produced is a true chemical compound. And this combination is alſo diſtinguiſhable by mutual ſaturation of the ingredients, whereas a mixture is poſſible with any proportion of them.

Theſe two kinds of combination are not ſo perfectly diſtinct that no exceptions are to be found in ſome circumſtances. Thus ſalt and water, in a certain determinate proportion, form a ſaline cryſtal. Here is ſaturation, but the chemical properties are ſcarcely changed. Tin and copper mix in any proportion, but the mixture has all its ſenſible qualities exceedingly different from thoſe of the ingredients. It is highly elaſtic, brittle, ſonorous, brilliant, and denſe; whereas the ingredients were ductile, ſoft, unelaſtic, and comparatively light.

Still, however, the diſtinction is uſeful when attended to.

The older chemiſts gave the name *mixt* to chemical compounds conſiſting of two ingredients, which we have never been able to reduce to ſimpler ingredients. Particles of a mixt, compounded with particles of another mixt, formed particles of a *compound*; the union

of two compounds formed a *decompound;* and the union of two decompounds formed a ſuperdecompound, &c. &c.

Modern chemiſts no longer uſe thoſe terms, but ſay, compounds of the firſt order, of the ſecond order, and ſo on.

GENERAL OBSERVATIONS.

This doctrine of chemical affinity is, unqueſtionably, the great and diſtinguiſhing principle of the ſcience, as the laws of motion are of mechanical philoſophy. The great advances made in the latter ſcience have derived but little aſſiſtance from the ſpeculations of ingenious men about the foundation of the *primary* laws of motion, and the intimate nature of moving forces. Indeed it has been greatly obſtructed by theſe ſpeculations, and by the attempts to explain all forces by impulſion. Kepler and Newton acquired their immortal fame by the diſcovery and exact deſcription of the *ſecondary* laws, the great or general facts, really obſerved in the operations of nature; which reſult, indeed, from the primary laws of motion, and are the modification of thoſe laws, by the circumſtances of the particular caſes. Such are the three laws of Kepler, and the great law of gravitation, which includes all the three.

I apprehend that chemical ſcience is, in like manner, obſtructed by ſpeculations about the principle of affinity, and, particularly, by the attempts of ingenious men to explain the chemical operations by attractions and repulſions. Even in mechanical philoſophy, attraction is a metaphorical term; and it is a mere figure of ſpeech to ſay, that a loadſtone *attracts* iron. But the reſemblance in effect to real attraction is very great; and I mean nothing by the term but this reſemblance. I mean only that the thing attracted actually approaches the attracter. But, in chemiſtry, even this is not obſerved in all the operations which we explain by attraction. In them all, the ſuppoſition is gratuitous, and, in many caſes it is falſe; for often the denſity

of the compound is leſs than the medium denſity of the ingredients. Therefore there is not that *local motion* and *approach* which alone authoriſe us to conſider the event as the effect of an attractive force. In chemiſtry, the metaphor, both in the expreſſion and the thought, is double. Union of things, formerly unconnected, reſembles, in a certain degree, the bringing into contact things formerly removed from each other: And this approximation reſembles the effect of a real attraction, an actual pulling the thing nearer to us with the hand.

I may venture to ſay that no man ever got a clear and really explicatory notion of a chemical combination by the help of attractions. When we have, in imagination, endowed a piece of copper wire with an attractive force, regulated by any law we pleaſe, and think that this will explain how it acts on a ſolution of nitrate of ſilver, or upon water, the mere communication of the piece of copper with the reſt of a galvanic circle changes the whole operation, and it either oxydates, or deoxydates, according to the end of the pile with which it communicates; and now, the particles which are ſuppoſed to exert an attractive force, or to attract, give evidence of repulſion, and, *vice verſâ*, although no change of ſituation or diſtance has been made in the particles attracted or repelled.

The chemiſt will do more wiſely, if he forego all ſpeculation about the ultimate internal action, and direct his whole attention to the external phenomena. Let him notice every general fact in the phenomenon, and claſs them according to their extent, and carefully note the ſubſtances which are the ſubjects of theſe general phenomena; theſe phenomena give him all that he can diſcover of the nature of thoſe ſubſtances. If he attempt to explain the internal motion of the atoms, which are the only true indications of the moving forces, he will find it impoſſible. He may fancy a certain attractive force, and then deduce certain motions from its action. But all the mathematicians in Europe are not qualified to explain a ſingle combination by theſe means. When he ſpeaks of attraction and affinity, let him affix no

further meaning to the terms than a certain faculty of combining; and when he ſays that the attraction of aquafortis for potaſh is greater than for ſilver, let him mean nothing more than this, that aquafortis combined with ſilver, will be ſeparated by potaſh, but ſilver will not ſeparate aquafortis from potaſh. We are altogether ignorant of the comparative intenſities of the moving forces, and what forces co-operate, and what counteract each other in chemical operations. Theſe depend as much on the circumſtances and manner of action, as on the intenſity of the forces. The only explanation to be received of a chemical phenomenon is, to ſhew that it is a caſe of a more general phenomenon already known. It is thus only that even a mechanical phenomenon is explained. The lunar irregularities are explained by ſhewing that they are caſes of univerſal gravitation. The truly chemical explanation is accompliſhed by ſhewing that the circumſtances of the phenomenon are preciſely ſuch as reſult from the acknowledged laws of that phenomenon from which we deduce the explanation.

Let chemical affinity be received as a firſt principle, which we cannot explain; any more than Newton could explain gravitation, and let us defer accounting for the laws of affinity, till we ſhall have eſtabliſhed ſuch a body of doctrine as he has eſtabliſhed concerning the laws of gravitation.

We may ſometimes have it in our power to aſſiſt the mind in conceiving a chemical phenomenon, by comparing it with a mechanical phenomenon. Thus, the chemical phenomena ariſing from galvaniſm may be conceived a little better, by comparing them with ſome magnetical phenomena. The cryſtallization of ſalts may be illuſtrated by comparing the particles to little magnets. But this is not explanation; nor is it even an illuſtration, unleſs on the ſuppoſition that we underſtand ſomething of the mechanical phenomena of magnetiſm.

The late diſſertations of Mr. Berthollet, on certain chemical affinities, are full of information, *purely chemical*, and are more free from

ill-judged applications of mechaniſm, than moſt other chemical writings. Yet they are not entirely free, and occaſion will be taken to mention ſome caſes in which his explanations are incompatible with the mechanical principles which he certainly adopts. Mr. Seguin, alſo, has made ſome very judicious obſervations, in his Diſſertation on the Changes of Chemical Affinity by Heat, publiſhed in different volumes of the *Annales de Chymie*.

EDITOR.

PART III.

CHEMICAL APPARATUS.

AGREEABLY to the plan which I laid out in beginning this courſe, we have finiſhed the firſt and ſecond diviſion of the General Doctrines of Chemiſtry, and ſhall now begin the third. Under this I propoſed to give an account of the nature and uſes of the apparatus or inſtruments uſually employed in performing the chemical operations and experiments. This is a ſubject which you are very well prepared to underſtand, by what has been already premiſed, in a general way, upon the ſubjects of heat and mixture. And it is neceſſary to take a view of it before we begin to conſider the objects of chemiſtry in a more particular manner.

But under the title of the Apparatus, or inſtruments, I do not propoſe to take under conſideration the nature and properties of water, of air, and of earth, as Dr. Boerhaave has done, who has given elaborate treatiſes upon theſe four ſubjects, under the title of inſtruments of chemiſtry. It is true that we apply heat to produce changes and effects upon different bodies, and that when we employ it in this way, it may be called, in metaphorical language, an inſtrument in our hands. It is alſo true that we make uſe of water on many occaſions, as an aſſiſtant by which we can reduce ſome bodies into a ſtate of flu-

idity, and mix them more effectually together. But if, upon this account, we muſt treat of fire and of water as inſtruments, for the ſame reaſons, and with the ſame propriety, I might bring moſt of the objects of chemiſtry under the ſame denomination: For, after we have learned by experiments the powers or qualities which belong to thoſe bodies, we take advantage of this knowledge, and employ them accordingly to ſerve our purpoſe on different occaſions. Thus, for gilding, mercury is an inſtrument; for ſeparating ſome metals from their ores, iron, and for others lead; various ſalts for ſeparating metals, and other bodies; black flux for reduction. But to treat of iron, and lead, and quickſilver, under the title of inſtruments of chemiſtry, would be a very improper and prepoſterous method. Dr. Boerhaave has been led into this method by the metaphorical language of ſome of the old ſyſtems of chemiſtry, in which it appears that earth, for what reaſon I cannot tell, has been likewiſe conſidered as an inſtrument, and is accordingly treated of as ſuch by the Doctor.

For my part, under the title of inſtruments, (I mean what is meant in common language) utenſils or furniture neceſſary for performing the chemical operations and experiments.

When we reflect upon the different particulars of which a chemical apparatus conſiſts, we find it may be conveniently divided into three parts. 1. Veſſels for containing the ſubjects upon which the effects of heat or mixture, or both together, are to be produced. 2. Means of producing heat. 3. Means for applying it in the beſt manner to the ſubjects on which it is to act, and for regulating its force.

We ſhall conſider the chemical apparatus according to this arrangement, and firſt therefore deſcribe the veſſels.

The variety which obtains among chemical veſſels, in conſequence of which they are fitted for different purpoſes, depends partly upon the *materials* of which they are made, and partly upon their *form*. I ſhall firſt mention the variety of materials which we find it neceſ-

ſary to employ in the conſtruction of chemical veſſels, and afterwards deſcribe their differences in point of form.

The remark with which Mr. Macquer ſets out in treating this ſubject is very juſt; that were it in our power to have veſſels completely adapted to chemical purpoſes, we ſhould wiſh them made of materials poſſeſſed of theſe qualities.

1*ſt*. We ſhould wiſh them tranſparent, that we might ſee diſtinctly what goes on in them.

2*dly*. Qualified to reſiſt the action of the moſt corroſive ſubſtances.

3*dly*. To bear ſudden alterations of heat without breaking.

4*thly*. Strong, to confine elaſtic vapours.

5*thly*. To bear violent heat without melting or being otherwiſe injured.

But, as there is no matter in nature poſſeſſed of all theſe qualities, chemiſts are under the neceſſity of conſtructing their veſſels of different materials, which have ſome of thoſe deſirable qualities, though they want the reſt; and in each operation or proceſs, they uſe veſſels made of materials which have the qualities more eſpecially neceſſary in that operation. The different materials are glaſs, metal, earthen ware.

Glaſs, when good, poſſeſſes the two firſt properties in ſufficient perfection. It is tranſparent, and not eaſily touched by corroſive ſubſtances. But glaſs veſſels neither bear ſudden alterations of heat without breaking, nor are they ſtrong, nor do they endure a ſtrong heat without melting. The moſt troubleſome imperfection is that inability to ſupport ſudden alterations of heat and cold, which they have in common with all brittle bodies, and which was formerly explained. The way to diminiſh this imperfection in glaſs veſſels, is to make glaſſes which are to endure heat as thin as is conſiſtent with moderate ſtrength. Thus, the ſurfaces will be more equally heated, and the whole glaſs more flexible, ſo that the one half may be heated before the other with leſs danger. Florence flaſks are extremely ſerviceable

in ſuch operations, being ſo thin, and of a glaſs that is remarkably ſtrong. It is a great advantage too, to be ſpherical where the heat is applied, becauſe this form is eaſieſt made thin, and equally ſo; and beſides, this form receives heat in a more equal and diffuſive manner. It is neceſſary too that the glaſs be well annealed. By annealing glaſs, is meant the expoſing it, while glowing hot, to a red heat, which is allowed gradually to waſte. While glaſs is of a low red heat, it has a great degree of ſoft toughneſs, will bear the blow of a hammer, and be dimpled rather than break. But if a thick glaſs be expoſed in this hot ſtate to cool air, the outer ſurface cools and contracts, and the ſoftneſs of the internal parts enables them to give way to this contraction, like a ductile metal, and the glaſs grows a little thicker. When the outer ſurface has thus grown cold and hard, the inſide commonly ſplits by contracting. If it remain whole, all the parts are on the ſtretch, and, in this ſtate, a ſlight touch of flint, hard ſand, or even hard ſteel, tears this ſtretched ſurface with facility, and the ſudden ſhock makes the vibration extend even to the outſide, and all flies in pieces.

To remove this imperfection, the glaſs is made to cool extremely ſlowly, ſo that all may contract alike, and take the regular and natural arrangement of the parts. The workmen never anneal funnels, becauſe they ſay that the inſide cools as faſt as the outſide.

When thus conſtructed, glaſs veſſels are ſufficiently ſecure, provided we be cautious to heat or cool them gradually, and they are uſeful in a great variety of operations. They are always preferred indeed where great heats or ſudden changes are not neceſſary, and where ſtrength is not required.

Metallic veſſels have the third and fourth qualities in perfection, which glaſs wants, viz. to bear ſudden viciſſitudes, and to be ſtrong. But they are deficient in all the reſt, for they are not tranſparent; they are liable to be corroded, and diſſolved by many active ſubſtances; nor do they ſupport a violent heat, without melting, or being

deſtroyed by it. The moſt troubleſome imperfection that attends them is that they are ſo liable to be corroded or ruſted, and to taint the ſubjects put into them. This is unfortunately the caſe in ſuch metals as are moſt uſually employed, namely, iron or copper. They are in ſome meaſure improved by tinning, but imperfectly. Beſides iron and copper in common uſe, lead and tin are uſed too in ſome particular operations, when a very moderate heat only is neceſſary, and when acrid ſubſtances are treated; as in the manufacture of alum, or copperas, becauſe they are leſs liable to corroſion and ruſting. For very nice purpoſes, when heat is neceſſary, and all neatneſs and cleanneſs muſt be ſtudied with ſcrupulous care, as in drying the gold after the depart, veſſels of platinum are uſed.

Earthen ware is the third and laſt matter employed in the conſtruction of chemical veſſels; and the beſt kinds of it poſſeſs ſome of the deſireable qualities in a high degree of perfection, eſpecially the power to withſtand violent heats, without melting, or otherwiſe ſuffering. The baſis, in general, is clay, which, when good, has properties that render it very convenient and uſeful for the formation of veſſels. Theſe properties are, ductility when moiſt, ſo as to be eaſily wrought into any form; and denſity, ſolidity, firmneſs, are acquired by burning; while, at the ſame time, it endures the moſt violent heat without further change. Theſe are the qualities of the very pure kinds, as pipe-clay, ſtourbridge, &c. Others are impure, and melt or vitrify. Of clay alone, very denſe and cloſe veſſels may be obtained, with ſufficient heat; but they are extremely liable to crack. This is more or leſs prevented by mixing the clay with ſubſtances that do not rarefy much by heat, and that divide, or render it more porous. Sand is a proper ſubſtance, taking care that it be of a kind which does not diſpoſe the pure clay to vitrification. Powdered flint, or quartz, are the beſt; alſo the fragments of clay veſſels, which have already ſerved, and become hard in the fire, are beaten into a coarſe powder for the ſame purpoſe. Black lead is one of the fineſt ingredients for this

mixture: But it makes a porous compoſition, which ſome melted matters readily ſoak through. A greater or larger quantity muſt be mixed according to the purpoſes intended. (Vide *Pott Berlin Mem.*) Where the moſt perfect cloſeneſs of texture is required, porcelain is choſen. This is a compoſition, and will be deſcribed afterwards; as will alſo Reaumur's porcelain, when we conſider the earths as the ſubjects of chemical examination.

For ſome very particular purpoſes, but of frequent occurrence, the chemiſt makes uſe of veſſels, called CUPLES, formed of bone-aſhes.

The variety of veſſels, in point of form, depends alſo on the uſes for which they are intended. To give a ſummary account of theſe uſes, I ſhall firſt remark, that, in the conduct of chemical experiments and operations, we have occaſion, either, 1*ſt*, To mix fluids with one another, or with ſolid ſubſtances, and often to promote their union and action on one another by moderate heat; or, 2*dly*, To melt ſolids together with a ſtrong heat; or, 3*dly*, To convert the whole or a part of the ſubject we are working upon into vapour.

One of theſe three modes of operation is practiſed in every chemical proceſs or operation; and all the veſſels we employ in our operations are fitted by their form to one of theſe three intentions. We may therefore divide them into,

1*ſt*. Veſſels for making mixtures and ſolutions.

2*dly*. Veſſels for fuſions.

3*dly*. Veſſels for vaporations.

Theſe diviſions comprehend all the veſſels employed in the operations of chemiſtry. There is one other ſet of veſſels, however, neceſſary to the chemiſt, and theſe are,

4*thly*. Veſſels for the preſervation of his materials and products.

Firſt, therefore, of the veſſels for making mixtures and ſolutions. There is a little variety in the forms of thoſe veſſels, in conſequence of ſome diverſity in the nature of the ſubjects, and in the practice of the operations. 1*ſt*, We have frequent occaſion to make mixtures in

which there are ſome ingredients conſiderably volatile, and part of which would evaporate, were they expoſed to the air. It is alſo proper, in making many mixtures, to promote the action of the ingredients on one another by gentle heat, or by digeſtion; which digeſtion cannot be performed in a common open veſſel, on account of the evaporation which would take place. Veſſels which ſerve theſe purpoſes are, the PHIALA CHEMICA, MATRASS, or BOLTHEAD, or veſſels approaching to that form, ſuch as flaſks. The propriety of this form is eaſily ſeen. Their oblong ſhape and ſmall orifice prevents the loſs by evaporation, and allows a conſiderable agitation, for promoting the mutual action of the contents, without any riſk of ſpilling. The bulb is made thin, to bear external or internal heat.

But ſometimes we apply greater heat to promote the action of the materials; ſuch a heat as raiſes them to vapours. As theſe vapours muſt be condenſed, and made to return into the veſſel, the chemiſt uſes an apparatus of veſſels, called a CIRCULATING APPARATUS, or PELICAN, from its diſtant reſemblance to a bird pecking its breaſt. The apparatus conſiſts of a tall pear-ſhaped veſſel, for receiving the ſubject. This has a broad-headed, or turnip-ſhaped capital, fitted into its mouth. The vapours riſe into this head, and are there condenſed, and would fall back through the riſing vapours, and condenſe them alſo; but this is avoided by making two pipes branch out from the lower ſide of the head, and reach pretty far down, where they are inſerted, ſloping into the ſides of the upright body. Thus, the fluid produced by condenſation in the head, runs down by the lateral pipes into the body. This operation is called CIRCULATION.

COHOBATION is merely the pouring back into the body of the matraſs the liquor which has been collected, by properly condenſing the vapours. Here we do by hand, and from time to time, what the circulating apparatus performs without our attendance.

In theſe ways of applying heat to our mixtures, however, we cannot raiſe it higher than the degree which the materials can bear

under the ordinary preſſure of the air. I told you formerly that Dr. Papin, an ingenious phyſician and chemiſt, has gone much farther, and has contrived a machine, by means of which we can apply to ſome of theſe mixtures a much greater heat than they can bear in ordinary circumſtances. Such is his DIGESTER formerly mentioned. In this machine the operation called DIGESTION is puſhed to the greateſt heat poſſible. You are doubtleſs aware that the operations with Papin's digeſter muſt always be conducted with caution, that accidents may not happen by the veſſel's burſting. The digeſter muſt be made of copper, ſoldered with hard ſolder, and its lid muſt be provided with a loaded valve, which will allow the vapour to eſcape when its elaſticity becomes hazardous. By loading the valve at the rate of ten, twenty, thirty, or forty pounds on a ſquare inch of the ſurface of the valve, we ſhall raiſe the heat of watery mixtures to about 236°, 254°, 270°, and 284°, of Fahrenheit's ſcale. By managing the fire ſo that the valve only puffs now and then, under theſe loads, we ſhall know pretty nearly the heat of the operation. The digeſter has long been uſed in Germany for the purpoſes of cookery, ſuch as for making ſtrong broths from bones and ſinews and the leſs delicate parts of meat.

There is another operation, in which, on account of the nature of the materials, which are leſs volatile, a ſtill greater heat is applied to make them act. The materials are laid *ſtratum ſuper ſtratum*, and preſſed cloſe together, into veſſels of a proper form, made of earthen ware, or other materials which will ſtand the red heat which is neceſſary for converting one of the ſubſtances into vapour, in which ſtate only it is able to penetrate and act upon the other in the manner we want. This is a proceſs chiefly practiſed in metallurgy. From ſome reſemblance between the arrangement of the materials, and the laying of bricks in mortar, this practice has been called CEMENTATION.

Other inſtruments employed by the chemiſt to facilitate mixture

and ſolution, are the mechanical inſtruments by which ſolid ſubſtances are broken into fragments, or pounded to powder, to prepare them for being diſſolved or mixed with others; or, if they are malleable, they are beaten into thin plates, or reduced to filings; all which is effected by means of mortars, ſieves, files, the hammer and anvil, &c.

In the laſt place, as it often happens that the matter we are employed in diſſolving does not diſſolve completely, but that a ſediment, or fæculency, or other undiſſolved matter remains, we purify the ſolution by filtration or by ſubſidence.

We muſt next conſider the veſſels employed in the operations in which ſolid materials are melted together with a ſtrong heat. Theſe are CRUCIBLES, which, by their taper form, and the triangular form of the brim, are fitted for precipitation, and for pouring out. Beſides the mere crucible, we muſt have covers, to keep out the coals or aſhes. A ſmaller crucible, inverted on the mouth of that containing the materials, is a very convenient cover. They muſt generally be ſet on pedeſtals of conſiderable height, that there may be abundance of glowing coals below as well as around them. They are lifted, for pouring, with a pair of crucible tongs, the ends of which are bent into a ſhape fitted for graſping the crucible, and holding it ſteadily. The contents are poured out into a mould, called an INGOT-MOULD, or into a conical braſs veſſel, ſhaped like an old-faſhioned beer glaſs. A common ſmall braſs mortar will anſwer the purpoſe very well. Theſe moulds are rubbed with chalk, or are ſmoked, to prevent adheſion.

The materials of crucibles are always earthy compoſitions, made with great care, as they are often expoſed to great heats. There are but two varieties in common uſe—the Heſſian, and the Ipſian, or Auſtrian, or black lead. The Heſſian are manufactured in immenſe numbers in the neighbourhood of Caſſel. They are compoſed of a very fine clay, mixed with a great proportion of ſand, and are baked

to a conſiderable degree of hardneſs. They ſtand a moſt violent heat without vitrification. Their great excellency is their compactneſs, not eaſily penetrated by the melted matters. Their defect, connected with this compactneſs, is a readineſs to ſplit, when a vein of colder air ſometimes gets at them while they are intenſely hot. The Auſtrian crucibles, compoſed of clay mixed with black lead, can ſcarcely be acted on by any materials contained in them; and they bear ſudden cooling much better than the Heſſian. But they are ſo porous, that ſome metalline glaſſes and ſlags ſoak through them like water. They are of moſt uſe for melting the moſt refractory metals. The ordinary method of preventing them from cracking is by doubling them, that is, ſetting one within another, with powdered clay between, or by luting without, with a coating of clay and ſand.

Theſe veſſels are employed for fuſion, or for operations that depend on it, when the heat required is conſiderable. For ſometimes others will do, as iron, for melting lead; glaſs, for ſome ſalts, &c.

We muſt next attend to veſſels contrived for containing thoſe bodies, or mixtures of bodies, which are to be converted in whole, or in part, into vapour.

I had lately an opportunity of remarking, that, in operations of this kind, we have one of two ends in view. We either wiſh to diſſipate the volatile parts, and to preſerve the fixed only; or we deſire to preſerve the volatile parts. The operation we perform, is, therefore, different in theſe two caſes, and is called in the firſt *evaporation*, and in the ſecond *diſtillation*, or *ſublimation*. In evaporation, the veſſels are open; in the other two, the vapour is confined, and directed from the veſſel in which it is formed into another, which is kept cool, where it is condenſed again. This is called DISTILLATION, when the vapours condenſe into a fluid; and SUBLIMATION, when they condenſe into a ſolid matter. The veſſels employed in all operations in which vapour is produced, may therefore be arranged into thoſe fitted for evaporations, and thoſe fitted for diſtillations or ſublimations.

The veſſels employed for evaporations are open veſſels. The materials and form may be different, according to the nature of the ſubject. If it is a mixture of water with other matters which have no corroſive quality with reſpect to metals, any metallic pan, or pot, or other veſſel, may ſerve. If the mixture be of a corroſive nature, the veſſels muſt be of glaſs, or earthen ware. The beſt form of glaſs veſſels for evaporations, is the globular; and, where there is an opportunity, they may be improved by hardening them in Mr. Reaumur's way. Or we may uſe veſſels of the beſt and hardeſt kinds of earthen ware. Excellent ones for this purpoſe are made by Mr. Wedgewood.

There are ſome caſes of evaporation, in which the heat muſt be exactly regulated. There are ſome ſubſtances which ſuffer from a briſk heat, as extracts, juices, or decoctions of plants; and which it is therefore proper to evaporate with as gentle warmth as poſſible, by inſenſible or ſpontaneous evaporation. If the utmoſt care be not taken, the extract, when brought to its proper degree of concentration, is generally black, and has a burnt taſte, even although the heat appears far below what would colour any vegetable ſubſtance. Here we muſt make amends for the mildneſs of the heat, and promote the evaporation, by ſpreading out ſuch matters in as broad a ſurface as poſſible. We therefore uſe, in this caſe, large flat diſhes of ſtone-ware, or porcelain; and the beſt way of uſing theſe is to place them in the mouth of a copper or iron veſſel containing water. Thus, the ſteam of the water will heat them equally, and ſecure them from breaking, and the heat will never riſe too high. This manner of applying heat is called the *balneum vaporis*. When the veſſel is ſet in boiling water, it is called *balneum mariæ*, and when in ſand, it is called *balneum arenæ*.

In ſome caſes of evaporation, when the heats required to produce vapour are very ſtrong, and metallic veſſels are improper, we uſe crucibles, as in evaporating ſome metallic ſubſtances themſelves.

To theſe particulars, with reſpect to the conduct of evaporation, we may add a method propoſed not long ago by Dr. Hales, for perform-

ing evaporation more quickly with a gentle heat. This is by blowing up ſhowers of air, as he expreſſes himſelf, through the liquor. This he propoſed, in order to haſten the diſtillation of ſea water; and he found it effectual for diſcharging certain bad ſmells or flavours from milk, &c. An inſtrument of this kind is ſold in London, to ſweeten ſtinking water at ſea, which it does, by evaporating the putrid ſteams. The inſtrument is merely a long pipe, terminating in a broad ſtreamer, like that of a garden watering pot. This is joined to the nozzle of a pair of bellows, and the ſtreamer is immerſed to the bottom of the fluid, and the bellows are worked, to force the air through the holes, in a great number of ſmall ſtreams. This contrivance was applied, on a very great ſcale, to the evaporation of water, in the manufacture of ſea ſalt; but, inſtead of promoting the evaporation, it was found to check it prodigiouſly. It muſt do ſo, becauſe, in this manufacture, a boiling heat is employed; and cold air ſhowered through the boiling water, muſt both cool the water, and condenſe the ſteam. When we come to conſider this manufacture, we ſhall mention much more effectual methods of applying the abſtracting power of air.

Let us next conſider the veſſels employed in diſtillation and ſublimation. I ſaid juſt now that we give theſe names to the operations in which we direct vapour into another veſſel, or another part of the veſſel, in which it is condenſed again by cold, and that the diſtinction is taken from the appearance of the condenſed matter; if fluid, diſtillation; if ſolid, ſublimation. I may alſo explain here a few more terms. The term, SPIRIT, is often applied to the fluids obtained by diſtillation; and the terms, SUBLIMATE, or FLOWERS, to the matters condenſed in ſublimation; the more fixed matter which remains behind being commonly, in both caſes, called the RESIDUUM, and ſometimes CAPUT MORTUUM, when ſolid and black.

To return to the veſſels—Thoſe employed in diſtillations have been ſuited to three different ways in which this operation has been conducted; and theſe three different ways got the names of diſtilla-

tion; 1. *per deſcenſum*, or downwards; 2. *per aſcenſum*, or upwards; 3. *ad latus*, to a ſide.

The firſt is, when the heat is applied above the ſubject, and the vapour is forced directly downwards, and condenſed in a part of the veſſel, kept cool below. See in chemical books, the manner of obtaining oil of cloves in this way. But it is laid aſide, and there is no operation of this kind in uſe at preſent, except the preparation or manufacture of tar, which reſembles this, and the ſeparation of zinc and mercury from the ore.

Diſtillatio per aſcenſum, is where the vapour is allowed to follow its natural diſpoſition, and to aſcend to ſome height upwards from the ſubject, and is then directed aſide into a cold cavity, where it is condenſed. For this a variety of veſſels have been contrived, the moſt common of which is the common copper ſtill, ſo well known that it needs little deſcription. It conſiſts of a BODY, a HEAD, and a REFRIGERATORY, or cooler. From the head iſſues a tube, inſerted into the upper end of another tube, which paſſes obliquely downwards through the refrigeratory, but without communicating with it. This laſt tube is often formed into a cylindrical ſpiral, like a cork-ſcrew, in order to increaſe the quantity of cold ſurface, to which the paſſing vapour may be applied for condenſation. The liquor produced trickles down the ſloping ſpiral, and is received into a proper veſſel. Some have them with different heads; and it is alſo common to have a water-bath-ſtill adapted to the common one: That is, the body of the ſtill is ſet into another body, diſtant from it a little way all round. Water is kept boiling in the outer veſſel, and then a moſt regular and uniform heat is applied to the ſubſtances contained in the inner body. This ſtill anſwers for the diſtillation of all ſuch ſubjects as are not diſpoſed to diſſolve or corrode the metal ſtill, or worm. But when we have corroſive acrid ſubſtances to deal with, or when we have a ſmall quantity of any matter, we uſe glaſs veſſels. And thoſe that reſemble the common ſtill moſt in form, are CUCURBITS, with alembics or capitals, and a receiver. Cu-

curbits are called BODIES by the Engliſh chemiſts. Theſe are made higher or lower. The two joints muſt be made tight, by lute, or ſlips of linen ſmeared with clammy matter, and then lapped round, over the joints.

But when glaſs veſſels are proper, either on account of the acrimony of the ſubject, or its ſmall quantity, or our deſire to ſee the phenomena, the veſſels moſt ſimple, and generally moſt convenient, and therefore moſt commonly uſed, are the RETORT and RECEIVER. The diſtillation performed in theſe is the *diſtillatio ad latus*. It is more convenient in the greateſt number of caſes, becauſe there is only one joint. Retorts are made of different heights, and may be placed differently. The materials are commonly introduced into retorts by a funnel. But for ſome operations, it is proper to have tubulated retorts; that is, having a tube formed at top, through which the ſubjects can be poured. The tube is then ſhut by a cork, or a ground glaſs ſtopper.

Glaſs retorts ſerve for a great variety of diſtillations, where moderate heat is ſufficient. If a ſtronger heat be requiſite, the retorts muſt be coated with clay, which helps to prevent both breaking and collapſing; or they muſt be uſed of earthen ware, like that of crucibles. Green glaſs coated is better. For particular purpoſes, it is convenient to have a ſort of iron retort, of two pieces.

The ordinary form of receivers has been globular, but a conical form, ending with a hemiſphere, is better. Sometimes a hole is bored in the neck, that the fumes may eſcape, which would otherwiſe burſt the veſſel. Some receivers have a tube joined to their bottom. Theſe are called ſpout receivers. In operations upon ſome very volatile ſubſtances, the older chemiſts imagined that a ſimple receiver was not ſufficient, and they uſed a ſeries of them, each of which was terminated in a wide tube, which fitted into the neck of the next. Such were called ADOPTERS. They are never uſed now, and a large receiver is much better. In very difficult caſes of this kind, ſome of theſe in-

termediate veſſels are found indiſpenſably neceſſary. Mr. Peter Woulfe, F. R. S. has contrived an apparatus for thoſe difficult diſtillations, which, by proper modifications, will obviate every difficulty. A particular deſcription of this apparatus would take up much of our time, and therefore I refer you to Phil. Tranſ. vol. 57. p. 517.

After, or during ſome diſtillations, we have occaſion to make uſe of a SEPARATORY, which is a glaſs veſſel, of a tall form, having a ſpout riſing from nearly the bottom, and rather exceeding the veſſel in height. It is plain that by this contrivance, the lower parts can be poured off, without diſturbing or being diſturbed by the upper parts.

For ſublimation, the veſſels employed are ſometimes the cucurbit and capital, becauſe, in many ſublimations, ſome liquor comes before the dry matter ſublimes. But in ſuch caſes, we more commonly uſe a retort with a wide and ſhort neck, and a receiver; for which purpoſe the conical form is much the beſt. For particular purpoſes we uſe an oblong or ſpherical glaſs veſſel,—a Florentine flaſk, or even a common phial; and ſometimes the aludels, or allodials, which reſemble the antique receivers which are mentioned above, are uſed in a ſeries.

Having now finiſhed the liſt of veſſels which are employed in chemical operations, there only remain to be mentioned thoſe neceſſary for the preſervation of many of the products of chemiſtry. Many of thoſe products are volatile, and require to be ſhut up cloſely in veſſels. Others are affected by the humidity of the air; and it happens, at the ſame time, that many of them are very corroſive, and deſtroy common corks. Such can be preſerved, therefore, only in glaſs veſſels, with glaſs ſtoppers, that ſhut them cloſe; and a number of ſuch phials are neceſſary.

It is alſo proper to ſubjoin to the enumeration of the veſſels, the mention of a means by which we alter them on ſome occaſions, by cutting off parts of them to fit them better to our purpoſe. This is done by an iron knife or rod made red hot. A ſmall ſcratch being firſt made on the edge of the glaſs, the red hot knife is applied there,

and preſently ſplits the glaſs in that part. By keeping the rod applied, and turning the glaſs very ſlowly under it, this crack may be conducted wherever we pleaſe, and the whole glaſs may be reduced to a narrow ſpiral ribbon. To cut thick bottle glaſs quite round, we may tie round it four threads of cotton-wick, and wet it with oil of turpentine, and ſet it on fire; and when it is burnt out, ſprinkle water on it, and it will ſeparate immediately.

Having given you ſuch remarks as may ſerve to direct you in the choice of the veſſels employed in chemical operations and experiments, we paſs to the ſecond part of this ſection, namely, the means for producing, and the way of managing the heats which are required in thoſe operations.

The means which the chemiſts, as well as others, employ for producing heat upon ordinary, and almoſt all occaſions, is the inflammation of combuſtible bodies, or the burning of fuel. But as there are ſeveral other ſources of heat beſide this, moſt of which have been uſed occaſionally for particular purpoſes, and ſome of which may be capable of producing ſingular effects; I ſhall enumerate them ſhortly, and juſt notice what are the purpoſes to which they have been applied, or which they may be capable of anſwering in chemiſtry.

The means by which various degrees of ſenſible heat are produced and communicated are, I think, ſeven:

1. That principle or power in animals which keeps them warm, and enables them to communicate heat to other bodies.
2. Friction or percuſſion of hard bodies.
3. Electricity.
4. Mixture.
5. Fermentation or putrefaction.
6. The ſun's rays.
7. Inflammation of fuel.

1. The heat produced by the firſt of theſe cauſes is employed for no chemical purpoſe, except the regulation of thermometers. The heat

of the perſon who makes them being always the ſame while in health, he can find this degree on a new thermometer, by putting it in his mouth for ſome time.

2. Friction or percuſſion of hard bodies is applied to ſome purpoſes in the arts and in common life; as for kindling gunpowder in the uſe of fire-arms, and to kindle fuel according to the Indian practice; but it is ſeldom uſed by the chemiſts except occaſionally for kindling fuel *.

3. The third means I mentioned, electricity, produces the electrical fire or flaſh which appears when the electrical fluid is greatly condenſed in its motion from one place to another. We thus produce very violent heats—heats perhaps ſuperior in violence and intenſity to any other whatever. This appears from their effects, ſuch as melting gold, and iron, and other metals in a moment, and even diſſipating them, or converting them into vapour, which penetrates the ſurface of the glaſs on which the experiment is made. But we can produce theſe heats by electricity, in ſuch quantity only as is ſufficient for heating very ſmall maſſes of matter. The reaſon why we ſucceed with theſe is, that the heat is produced not gradually, but inſtantaneouſly; ſo inſtantaneouſly, that there is not time for any part of it to be loſt by being communicated to other bodies before it produce its effect. Hence money has been melted in the pocket, and a ſword in the ſcabbard. Dr. Lind melted ſteel under water by an electric flaſh.

* The experiments of Mr. Pictet of Geneva, on the heat produced by moderate friction in the air and *in vacuo*, are curious, and promiſe to lead to ſome further knowledge of heat. He publiſhed an account of them in the Journal de Phyſique, and alſo in a ſmall volume, Geneva, 1790.

Alſo the great experiments made by Count Rumford on the production of heat by the friction of metals, are peculiarly valuable, and lead to much ſpeculation concerning heat. See Phil. Tranſ. 1798.

It is worthy of notice that this production of heat is accompanied with light, even in caſes where it cannot be aſcribed to combuſtion, as in the colliſion or friction of pieces of quartz, and other ſiliceous and hard bodies. EDITOR.

Such facts once ſuggeſted the notion that the fuſion occaſioned by electricity was a cold fuſion ; but this is a miſtake. A wire too thick to be melted may be made red hot. Electricity, therefore, is a moſt powerful means for producing heat, and produces it in a very particular manner. However, as chemiſts have not much attended to theſe particulars, merely as ſources of heat, the inſtruments for exciting electricity would be improperly conſidered as a part of the chemical apparatus.

4. The fourth means of producing heat is the mixing particular bodies. This is not applied to uſe in the practice of chemiſtry for producing heats, becauſe the heats produced in this way are all momentary and tranſient. Some of them ar euſed by the fire-eaters and raree-ſhow-men for ſuddenly kindling tow, &c. They will be mentioned in the courſe of our examination of chemical ſubſtances.

5. The fifth means I mentioned as ſometimes employed, is by taking advantage of the heat generated in the fermentation or putrefaction of vegetable and animal ſubſtances. Fermentation and putrefaction are changes to which animal and vegetable ſubſtances are liable, when preſerved from exceſſive cold, and tempered with a moderate quantity of moiſture. Theſe changes come on gradually, and require ſome time to complete them. During this time, the contexture of the ſubſtances is deſtroyed, their principles enter into new combinations, and the oſtenſible chemical properties of the matter are quite changed. While this change goes on, there is a conſtant emiſſion of heat from the maſs, which can therefore be employed to communicate this heat to other bodies. The heat produced in this way is not ſtrong, but very laſting, and cheaply procured. Hence it is uſed for promoting vegetation ; and, in Egypt, it has been long in uſe for the hatching of eggs, in particular parts of the country where they rear incredible numbers of fowls, (Vide Reaumur). And in chemiſtry, it has been often employed for the digeſtion of various mixtures, to which purpoſe it is remarkably adapted. In ſome chemical arts too, the heat thus pro-

duced is employed as a digeſting heat, as in the manufacture of verdigreaſe and of white lead.

The ſubjects which have been commonly uſed are horſe dung, and the oak bark of the tanners, either of which is capable of maintaining a heat equal to 120° Fahrenheit or more. The firſt of theſe acquires this heat ſooneſt, and is capable of becoming hotter than the other; but the inteſtine motion, and the generation of heat is ſooner over, commonly in about a month. The heat produced by the putrefaction of bark is more gentle, but more laſting, ſo as to continue, if properly managed, for ſome months. The particulars to be attended to in uſing this means of producing heat, are, firſt, that there be a proper degree of moiſture. If there be either too much or too little, the fermentation will not go on properly, and little or no heat will be produced. Secondly, that a proper quantity of materials be gathered together in one compact heap. The reaſon why a ſmall quantity, or a heap that is not compact, will not anſwer the purpoſe, is that the ſurface of the maſs, which is in contact with the ſurrounding air, or other cold bodies, bears ſo large a proportion to the contents, that the heat is carried off as faſt as it is produced within. But if the heap be conſiderable, and properly compacted, the diſſipation and waſte of the heat is diminiſhed, and it is accumulated in greater quantity in the fermenting materials. In ſome caſes, when very large quantities of tender vegetable ſubſtances are heaped up together, and a due degree of moiſture conſpires to promote this inteſtine motion, the waſte of heat is ſo ſmall, and its extrication ſo copious and quick, that the maſs is conſumed by it, as is well known to have often happened to ricks of hay, when put up too green or wet, ſo that they contained moiſture enough to admit of putrefaction.

6. The heat of the ſun's rays. Theſe, when a ſubject is fully expoſed to them in a ſheltered ſituation, give a heat that is not inconſiderable, equal even to that which is commonly produced by putrefaction. *(See Note 25. at the end of the Volume.)* The heat of the ſun's rays

may be uſefully employed in digeſtions, &c. but it is not ſo conſtant as that obtained by putrefaction. As the heat thus produced is not attended by humid ſteams, it is uſeful alſo in drying many bodies, or diſſipating their moiſture.

The rays of the ſun have a particular effect upon many coloured ſubſtances, in diſcharging the colour. They are a ſevere trial of dyes. Moreover, ſome bodies are remarkably changed in their compoſition by being expoſed to the light of the ſun.

Such are the effects of the rays of the ſun, in their ordinary ſtate; but they are remarkable on account of the amazing violence and intenſity to which we can increaſe their effect, by condenſing them, or directing a great number to fall upon the ſame ſpot. This is effected by concave mirrors, or convex lenſes. Inſtruments of both theſe kinds have been executed, at different times, which produced effects that appeared aſtoniſhing when they were new; ſuch as melting in a moment many earths and ſtones, which were reckoned before perfectly unfuſible; converting into ſmoke, gold, ſilver, and other metals, which were imagined before to be perfectly fixed. See ſome of thoſe facts related by Boerhaave, and in the Memoirs of the Academy of Sciences of Paris. The moſt famous of thoſe inſtruments are a ſpeculum, made by the Viletti of Lyons, a father and two ſons, and a lenſe, made by the celebrated Baron von Tchirnhans *. The diameter of the ſpeculum is 43 inches, the diameter of the focus half an inch, the diſtance three feet, its weight 400 pounds French. The lens belonged to the regent Duke of Orleans, and with it were made the experiments

* Both of theſe ways of kindling fire were known to the ancients. Euclid, in his Catoptrics, mentions expreſsly this effect of a ſpherical ſpeculum, only miſtakes the ſituation of the focus. Plutarch alſo, in his life of Numa, ſays that the veſtal virgins kindled their fires *Scaphiis*. The ſtratagem of Strepſiades, in Ariſtophanes's comedy, for burning the documents of his debts, by a globe of glaſs filled with water, gathering the ſun's rays on them, is a proof of the other point.

EDITOR.

related by Homberg and Geoffroy, in the Memoirs of the Academy. The diameter of it is 33 inches, the focus at the diſtance of 12 feet, and the diameter of the focus 1½ inch. The effects of the ſpeculum were, however, much greater than thoſe of the lens, and for this plain reaſon, that the image of the ſun is formed by it at a ſmall diſtance, and therefore not ſo broad, and the rays are therefore condenſed into leſs ſpace. For this reaſon ſpeculums have more effect, in general, than lenſes *. Specula of different ſizes are not ſo different from one another in power as we might imagine. This will not appear ſurpriſing, when we conſider that, ſuppoſing the exactneſs of workmanſhip equal, the rays will be equally condenſed by every ſpeculum. The diameter or breadth of a ſpeculum is always proportioned to the ſphere on which it is ground. A ſpherical ſurface collects the light in the focus, only in ſo far as it coincides with the apex of a paraboloïd, and it happens to be but a ſmall portion of a ſphere that coincides with it. The ſpeculum is moſt powerful—the lens moſt convenient, becauſe the ſpeculum directs the rays upwards towards the ſun. The object cannot be ſo conveniently expoſed to them in that ſituation, and it interrupts light. It is uſual with lenſes, to employ another lens, of ⅓ of the diameter and focal diſtance, to collect the rays ſtill more. Thus the moſt intenſe heats are produced.

There are, in the Memoirs of the Academy, papers upon both theſe kinds of inſtruments, and many projects are propoſed, and experiments related, with a view to conſtruct them with greater facility and perfection, by Mr. Buffon. This gentleman may be ſaid to have reinvented the contrivance put in practice by Archimedes, when he ſet fire to the ſhips of the enemy's fleet, employed in the ſiege of Syracuſe, *(Vide Encyclopedie)*. This ſtory was long conſidered as one of thoſe marvellous fables which are related of the great men of

* Lenſes are inferior, for another reaſon. They abſorb or intercept a great proportion of the heat. This is evident from Scheele's experiments.

EDITOR.

remote antiquity. But the experiments of M. Buffon have ſhewn that the thing is poſſible; and Tzetzes, a Greek author, deſcribes the contrivance of Archimedes, which proves exactly the ſame as the invention of Mr. Buffon *. I call it invention, as I do not ſuppoſe he was acquainted with the Greek author, otherwiſe he would have acknowledged the hint he got from him. Mr. Buffon employed a great number of ſmall glaſs ſpeculums, of 6, 7, and 8 inches broad, framing them ſo together that their ſeparate reflections united at a convenient diſtance. With 40, he kindled deal boards at 66 feet diſtance, and with 128, he did this at the diſtance of 150 feet. With 45, he melted tin at 20 feet diſtance, and with 117, he melted ſilver. At another time he kindled wood at 200 feet, melted tin at 150, lead at 130, and ſilver at 60, employing 168 mirrors. His contrivance had the eminent advantage of directing the light downwards, which made all experiments eaſy.

7. We are next to conſider fuel, as the moſt eaſy and manageable means of producing heat. We can eaſily manage this ſource of heat, ſo as to have all the different heats under command, from the moſt mild and gentle, to one approaching, in force, to that occaſioned by the collected rays of the ſun.

This variety in the effect of fuel depends upon the various nature of the fuel itſelf, and upon the management or command of its inflammation. With regard to the variety among fuels, it is very

* Zonaras mentions another inſtance of this contrivance. Proclus burnt the ſhips of Vitellianus, at Conſtantinople, in the ſame way. This was in the year 514. Kircher undertook to prove the truth of thoſe ancient ſtories by experiment, and, following the deſcription by Tzetzes, of plain mirrors turning on hinges, (γίγγλυμοις) he produced a very great heat by five mirrors, which he made to unite their reflection at 100 feet diſtance. Vitellio quotes a paſſage from Authemius, the architect of Juſtinian, mentioning his having kindled wood by 24 mirrors. This work of Authemius is now loſt, and never has been printed. Vitellio had no inducement whatever to falſify.

EDITOR.

great. But we ſhall notice only the moſt remarkable differences among the fuels commonly uſed. They may be reduced to five diviſions. The firſt may comprehend the fluid inflammables; to the ſecond we may refer peat or turf; to the third charcoal of wood; to the fourth pit-coal charred; and to the fifth wood, or pit-coal, in a crude ſtate, and capable of yielding a copious and bright flame.

1ſt. The fluid inflammables are conſidered as diſtinct from the ſolid, on this account, that they are capable of burning upon a wick, and become, in this way, the moſt manageable ſources of heat; though, on account of their price, they are never employed for producing it in great quantity, and are only uſed when a gentle degree, or a ſmall quantity of heat, is ſufficient. The ſpecies which belong to this claſs are ſpirit of wine, and different oils.

The firſt of theſe, ſpirit of wine, when pure and free of water, is as convenient and manageable a fuel for producing moderate or gentle heats as can be deſired. Its flame is perfectly clean, and free from any kind of ſoot; it can eaſily be made to burn ſlower or faſter, and to produce leſs or more heat, by changing the ſize or number of the wicks upon which it burns; for, as long as theſe are fed with ſpirit, in a proper manner, they continue to yield flame of preciſely the ſame ſtrength. The cotton, or other materials of which the wick is compoſed, is not ſcorched or conſumed in the leaſt, becauſe the ſpirit with which it is conſtantly ſoaked, is incapable of becoming hotter than 174° Fahrenheit, which is conſiderably below the heat of boiling water. It is only the vapour that ariſes from it which is hotter, and this too only in its outer parts that are moſt remote from the wick, and where only the inflammation is going on, in conſequence of communication and contact with the air. At the ſame time, as the ſpirit is totally volatile, it does not leave any fixed matter, which, by being accumulated on the wick, might render it foul, and fill up its pores. The wick, therefore, continues to imbibe the ſpirit as freely after ſome time as it did at the firſt. Theſe are the qualities of pure

ſpirit of wine as a fuel. But theſe qualities belong only to a ſpirit that is very pure. If, on the contrary, the ſpirit is weak, or contain water, the water, being leſs volatile, does not evaporate ſo faſt from the wick, as the more ſpiritous part; and the wick becomes, after ſome time, ſo much ſoaked with water, that it does not imbibe the ſpirit properly. The flame becomes much weaker, or is altogether extinguiſhed. When ſpirit of wine is uſed as a fuel, therefore, it ought to be made as ſtrong, or free from water as poſſible.

Oil, although fluid like ſpirit of wine, and capable of burning in a ſimilar manner, is not ſo convenient in many reſpects. It is diſpoſed to emit ſoot; and this, applying itſelf to the bottom of the veſſel expoſed to it, and increaſing in thickneſs, forms by degrees, a ſoft and ſpongy medium, through which heat is not ſo freely and quickly tranſmitted. This was obſerved by Muſchenbroeck, in his experiments upon the expanſions of metalline rods heated by lamps. It is true that we can prevent this entirely, by uſing very ſmall wicks, and increaſing the number, if neceſſary, to produce the heat required. Or, we may employ one of thoſe lamps, in which a ſtream of air is allowed to riſe through the middle of the flame, or to paſs over its ſurface with ſuch velocity as to produce a more complete inflammation than ordinary. But we ſhall be as much embarraſſed in another way, for the oils commonly uſed, being capable of aſſuming a heat greatly above that of boiling water, ſcorch and burn the wick, and change its texture, ſo that it does not imbibe the oil ſo faſt as before. Some have attempted a remedy, by making the wick of incombuſtible materials, as aſbeſtos, or wire; but ſtill, as the oil does not totally evaporate, but leaves a ſmall quantity of groſs fixed carbonaceous matter, this, conſtantly accumulating, clogs the wick to ſuch a degree, that the oil cannot aſcend, the flames become weaker, and, in ſome caſes, are entirely extinguiſhed. There is, however, a difference among the different oils in this reſpect; ſome being more totally volatile and inflammable than others. But the beſt are troubleſome in this way,

and the only remedy is to change the wicks often, though we can hardly do this and be ſure of keeping always an equal flame.

The ſecond kind of fuel mentioned, peat, is ſo ſpongy, that, compared with the more ſolid fuels, it is unfit to be employed for producing very ſtrong heats. It is too bulky for this—We cannot put into a furnace, at a time, a quantity that correſponds with the quick conſumption that muſt neceſſarily go on when the heat is violent. There is no doubt a great difference in this reſpect among different kinds of this fuel; but this is the general character of it. However, when we deſire to produce and keep up, by means of cheap fuel, an extremely mild gentle heat, we can hardly uſe any thing better than peat. But it is beſt to have it previouſly charred, that is, ſcorched, or burnt to black coal. The advantages gained by charring it will be preſently explained. When prepared for uſe in that manner, it is capable of being made to burn more ſlowly and gently, or will bear, without being extinguiſhed altogether, a greater diminution of the quantity of air with which it is ſupplied, than any other of the ſolid fuels. Dr. Boerhaave found it extremely convenient and manageable, in his *Furnus Studioſorum*.

3*d*. The next fuel in order is the charcoal of wood. This is prepared by piling up billets of wood into a pyramidal heap, with ſeveral ſpiracles, or flues, formed through the pile. Chips and bruſhwood are put into thoſe below, and the whole is ſo conſtructed, that, when kindled, it kindles almoſt over the whole pile in a very ſhort time. It would burſt out into a blaze, and be quickly conſumed to aſhes, were it not covered all over with earth, or clay, beaten cloſe, leaving openings at all the ſpiracles. Theſe are carefully watched; and, whenever the white watery ſmoke is obſerved to be ſucceeded by thin blue and tranſparent ſmoke, the hole is immediately ſtopped—this being the indication of all the watery vapour being gone, and the burning of the true coaly matter commencing. Thus is a pretty ſtrong red heat raiſed through the whole maſs, and all the volatile matters are diſſi-

pated by it, and nothing now remains but the charcoal. The holes being all ſtopped in ſucceſſion, as this change of the ſmoke is obſerved, the fire goes out for want of air. The pile is now allowed to cool—This requires many days: For, charcoal being a very bad conductor of heat, the pile long remains red hot in the centre, and, if opened in this ſtate, would inſtantly burn with fury.

Small quantities may be procured at any time, by burning wood in cloſe veſſels. Little pieces may be very finely prepared, by plunging the wood in lead melted and red hot.

This is the chief fuel uſed by the chemiſts abroad, and has many good properties. It kindles quickly, emits few watery or other vapours while burning, and when conſumed, leaves few aſhes, and thoſe very light. They are, therefore, eaſily blown away ſo that the fire continues open, or pervious to the current of air, which muſt paſs through it, to keep it burning. This ſort of fuel, too, is capable of producing as intenſe a heat as can be obtained by any; but in thoſe violent heats it is quickly conſumed, and needs to be frequently ſupplied.

4th. Foſſil coals charred, called cinders, or coaks, have, in many reſpects, the ſame properties as charcoal of wood; as kindling more readily in furnaces than when they are not charred, and not emitting watery, or other groſs ſmoke, while they burn. This ſort of charcoal is even greatly ſuperior to the other in ſome properties. It is a much ſtronger fuel, or contains the inflammable matter in greater quantity, or in a more condenſed ſtate. It is, therefore, conſumed much more ſlowly on all occaſions, and particularly when employed for producing intenſe melting heats. The only inconveniences that attend it, are, that, as it conſumes, it leaves much more aſhes than the other *, and theſe much heavier too, which are, therefore, liable to

* I don't know that this is the caſe with the fineſt pit-coal. I have ſeen ſome of the fineſt Newcaſtle coals, which did not leave $\frac{1}{50}$th of their weight of aſhes, and even theſe did not ſeem entirely conſumed. EDITOR.

collect in ſuch quantity as to obſtruct the free paſſage of air through the fire; and further, that when the heat is very intenſe, theſe aſhes are diſpoſed to melt or vitrify into a tenacious droſſy ſubſtance, which clogs the grate, the ſides of the furnace, and the veſſels. This laſt inconvenience is only troubleſome, however, when the heat required is very intenſe. In ordinary heats, the aſhes do not melt, and, though they are more copious and heavy than thoſe of charcoal of wood, they ſeldom choke up the fire conſiderably, unleſs the bars of the grate be too cloſe together.

This fuel, therefore, is preferable, in moſt caſes, to the charcoal of wood, on account of its burning much longer, or giving much more heat before it is conſumed. The heat produced by equal quantities by weight of pit coal, wood-charcoal, and wood itſelf, are nearly in the proportion of 5, 4, and 3. The reaſon why both theſe kinds of charcoal are preferred, on moſt occaſions, in experimental chemiſtry, to the crude wood or foſſil coal from which they are produced, is that the crude fuels are deprived, by charring, of a conſiderable quantity of water, and ſome other volatile principles, which are evaporated during the proceſs of charring, in the form of ſooty ſmoke or flame. Theſe volatile parts, while they remain in the fuel, make it unfit (or leſs fit) for many purpoſes in chemiſtry. For, beſides obſtructing the vents with ſooty matter, they require much heat to evaporate them; and therefore the heat of the furnace in which they are burnt is much diminiſhed and waſted by every addition of freſh fuel, until the freſh fuel is completely inflamed, and reſtores the heat to its former ſtrength. But theſe great and ſudden variations of the heat of a furnace are quite inconvenient in moſt chemical proceſſes. In the greater number of chemical operations, therefore, it is much more convenient to uſe charred fuel, than the ſame fuel in its natural ſtate.

There are, at the ſame time, ſome kinds of foſſil coal, which are exceptions to what has now been delivered in general. We meet with ſome of them that leave a ſmaller proportion of aſhes than others, and

the aſhes of ſome are not ſo liable to melt in violent heats. There is one ſpecies too, ſuch as the Kilkenny coal of Ireland, and which occurs likewiſe in ſome parts of this country, that does not contain any ſenſible quantity of water, or other ſuch volatile principles. But this may be called a ſort of native charcoal. It has the appearance of ordinary coal, but, when thrown into the fire, does not emit ſmoke or ſoot. It merely becomes red, gives a ſubtile blue flame, and conſumes like charcoal; only it laſts ſurpriſingly long, or continues to give heat for a very long time before it is totally conſumed. But it cannot be made to burn ſo as to produce a gentle heat. If not in conſiderable quantity, and violently heated, it is ſoon extinguiſhed.

In uſing this kind of fuel, it is proper to be on our guard againſt the dangerous nature of the burnt air which ariſes from charcoal of all kinds. Charcoal burns without viſible ſmoke. The air ariſing from it appears to the eye as pure and as clean as common air. Hence it is much uſed abroad by thoſe who are ſtudious of neatneſs and cleanlineſs in their apartments. But this very circumſtance ſhould make us more watchful againſt its effects, which may prove dangerous, in the higheſt degree, before we are aware of it. The air ariſing from common crude fuel is no doubt as bad, but the ſmoke renders it diſagreeable before it becomes dangerous. The firſt ſenſation is a ſlight ſenſe of weakneſs; the limbs ſeem to require a little attention, to prevent falling. A ſlight giddineſs, accompanied by a diſtinct feeling of a fluſh or glow in the face and neck. Soon after, the perſon becomes drowſy, would ſit down, but commonly falls on the floor, inſenſible of all about him, and breathes ſtrong, ſnoring as in an apoplexy. If the perſon is alarmed in time, and eſcapes into the open air, he is commonly ſeized with a violent headach, which gradually abates.

But when the effect is completed, as above deſcribed, death very ſoon enſues, unleſs relief be obtained. There is uſually a foaming at the mouth, a great fluſh or ſuffuſion of blood over the face and neck,

and every indication of an oppreſſion of the brain by this accumulation of blood. The moſt ſucceſsful treatment is to take off a quantity of blood immediately, and throw cold water on the head repeatedly. A ſtrong ſtimulus, ſuch as hartſhorn, applied to the ſoles of the feet, has alſo a very good effect.

To return to the fuels—

The *5th* and *laſt* kind of fuel is wood, or foſſil coal, in their crude ſtate, which it is proper to diſtinguiſh from the charcoals of the ſame ſubſtances. The difference conſiſts in their giving a copious and bright flame, when plenty of air is admitted to them, in conſequence of which they muſt be conſidered as fuels very different from charcoal, and adapted to different purpoſes.

I had occaſion formerly to remark, when treating of inflammation, that flame is produced from thoſe ſubſtances only, which are either totally volatile when heat is applied to them, or which contain a quantity of combuſtible matter that is volatile or eaſily convertible into vapour by heat; and that flame is nothing elſe but this vapour ſet on fire, or which becomes inflamed as faſt as it ariſes from the body which affords it.

Of this nature, therefore, is the flame of wood and foſſil coal, when they are burnt in their crude ſtate. Theſe fuels contain a quantity of inflammable matter that is volatile, and which, when a moderate and ſtifled heat is applied to them, evaporates in the form of oily and ſooty vapours and ſmoke, and diminiſhes the heat inſtead of increaſing it. But if they are expoſed to a ſtronger heat, and air is freely admitted, the ſooty vapours are ſuddenly ſet on fire, or become flame, and continue afterwards to burn as faſt as they ariſe from the wood or coal, in conſequence of which they produce a great heat.

Such is the nature of flame and of flaming fuel; and from this account of it, you will perceive that it is applicable only to certain purpoſes, and is unfit for the more common uſes in experimental chemiſtry. For, in the firſt place, the greater number of our experi-

ments and proceſſes do not require an intenſe and violent heat, but a moderate and continued heat, either kept to the ſame degree for ſome time, or ſlowly and gradually increaſed. This ſort of heat is eaſily commanded by means of charred fuel, which burns faſter or ſlower, according to the quantity of air allowed to paſs through it, and will continue to burn with a very ſmall quantity of air (but more ſlowly) as well as when much air is admitted. Whereas flaming fluel cannot be managed in this manner. If little air be admitted, it gives no flame, but ſooty vapour, and a diminution of heat. And if much air be admitted to make thoſe vapours break out into flame, the heat is too violent.

Theſe flaming fuels, however, have their particular uſes, for which the others are far leſs proper. For it is a fact, that flame, when produced in great quantity, and made to burn violently, by mixing it with a proper quantity of freſh air, by driving it on the ſubject, and throwing it into whirls and eddies, which mix the air with every part of the hot vapour, gives a moſt intenſe heat. This proceeds from the vaporous nature of flame, and the perfect miſcibility of it with the air. As the immediate contact and action of air is neceſſary to the burning of every combuſtible body, ſo the air, when properly applied, acts with far greater advantage on flame, than on the ſolid and fixed inflammable bodies: For when air is applied to theſe laſt, it can only act on their ſurface, or the particles of them that are outermoſt; whereas flame being a vapour or elaſtic fluid, the air, by proper contrivances, can be intimately mixed with it, and made to act on every part of it, external and internal, at the ſame time. The great power of flame, which is the conſequence of this, does not appear when we try ſmall quantities of it, and allow it to burn quietly, becauſe the air is not intimately mixed with it, but acts only on the outſide, and the quantity of burning matter in the ſurface of a ſmall flame is too ſmall to produce much effect. But when flame is produced in large quantity, and is properly mixed and agitated with air, its power to heat bodies is im-

menſely increaſed. It is therefore peculiarly proper for heating large quantities of matter to a violent degree, eſpecially if the contact of ſolid fuel with ſuch matter is inconvenient. Flaming fuel is uſed for this reaſon in many operations performed on large quantities of metal, or metallic minerals, in the making of glaſs, and in the baking or burning of all kinds of earthen ware. The potters kiln is a cylindrical cavity, filled from the bottom to the top with columns of ware. The only interſtices are thoſe that are left between the columns; and the flame, when produced in ſufficient quantity, proves a torrent of liquid fire, conſtantly flowing up through the whole of thoſe interſtices, and heats the whole pile in an equal manner.

Flaming fuel is alſo proper in many works or manufactories in which much fuel is conſumed, as in breweries, diſtilleries, and the like. In ſuch works, it is evidently worth while to contrive the furnaces ſo that heat may be obtained from the volatile parts of the fuel, as well as from the fixed; for when this is done, leſs fuel ſerves the purpoſe than would otherwiſe be neceſſary. But this is little attended to, or ill underſtood, in many of thoſe manufactories. It is not uncommon to ſee vaſt clouds of black ſmoke and vapour coming out of their vents. This happens in conſequence of their throwing too large a quantity of crude fuel into the furnace at once. The heat is not ſufficient to inflame it quickly, and the conſequence is a great loſs of heat.

Theſe are the principal varieties which occur in fuels, ſo far as they are fitted to produce more or leſs violent heat, or for heating bodies in different ways. There are differences too among them, in conſequence of which they produce other changes upon bodies in contact with them, than merely heating them more or leſs. This depends either on introducing ſomething or taking ſomething away. But I ſhall notice this when treating of the particular bodies which are diſpoſed to produce effects in this way.

The means of managing and commanding inflammation are now to be conſidered under the article furnaces.

The neceſſary requiſites of every furnace will occur, if we conſider the purpoſe of the contrivance in general, together with the nature of fuel and inflammation. Our purpoſe, for inſtance, in the contrivance of furnaces, is to confine the heat as much as poſſible to the ſubject, that it be not waſted, nor prove troubleſome to the operator. There is therefore a chamber, or cavity, to contain the burning fuel, called the FIRE-PLACE, the ſides of which are of ſome thickneſs, and the materials ſuch as withſtand the heat. There muſt likewiſe be a door, or opening into this chamber, at which freſh fuel may be introduced. And it muſt not be quite cloſe above; it muſt have a VENT towards the upper part, to convey away the ſmoke and heated burnt air, otherwiſe the fire would be quickly extinguiſhed. It is next obvious, that any fuel which we can employ in ordinary furnaces muſt produce aſhes, which would collect and choke up the fire. It is neceſſary, therefore, that the fuel lie on a grate through which the aſhes may fall in proportion as they are formed or collected in any quantity. Under the grate it is convenient to have another chamber for receiving theſe aſhes, an ASH-PIT; and at the ſide of this chamber, another door to take them out, and likewiſe to allow freſh air to flow up into the furnace, to ſupply the place of that which is paſſing through and going out at the vent. Theſe are neceſſary parts of every ſtructure called a furnace; and it is evident, that in ſuch a ſtructure inflammation will go on, or fuel burn, with conſiderable rapidity. The two requiſites to the inflammation of combuſtible bodies, are a certain degree of heat once excited in the burning body, and a conſtant renewal of the air in contact with it. The firſt of theſe conditions is obtained when the fuel is once ſet on fire, ſeeing that, in burning, it produces a great deal of heat, by which it is both kept hot itſelf, and communicates heat to the ſurrounding bodies. The ſecond condition is a neceſſary conſequence of the ſtructure of the furnace, for no ſooner is the fire well lighted than there is a motion of the air upwards through the furnace with conſiderable velocity. The air in the fire-place, by being

heated, is expanded, made rarer and lighter, and it is therefore puſhed upwards by the cooler and heavier external air, which communicates with it by means of the door of the aſh-pit and grate; and thus it will be made to flow in a ſtream through the vent. Its place is ſupplied by freſh air riſing up through the grate, and fuel. No ſooner does this freſh air touch the fuel, than it is heated, rarefied, and puſhed up in its turn, ſo that there is a conſtant ſtream of air flowing up through the furnace, as long as the fuel laſts or the furnace continues warm. The burning of any inflammable body in any manner whatever, is always attended with an aſcent of air like this, and hence the aſcent of ſmoke and flame; but it is moſt remarkable in a furnace in which fuel is allowed to burn freely, becauſe the external air, not having admittance to the ſpace above the fire, except through the grate and fire itſelf, is more heated and rarefied when it has aſcended through it, than the air which gets admiſſion above an ordinary fire, which has a free communication with the ſurrounding air, and is conſtantly mixing with it. It is plain, that the taller we make the chimney or vent of a furnace, there is a longer column of hot and light air in it, and therefore a greater preſſure upwards. Air ruſhes in more violently by this increaſed preponderancy of the air of the laboratory; this conſumes more fuel in the ſame time, and therefore excites a ſtill greater heat. In the iron ſmelting furnaces, they find the produce nearly in proportion to the number of cubic feet of air thrown into the fuel.

The venting of chimneys depends on the ſame principles, and the accidents to which it is liable, or bad venting, will be beſt remedied by a perſon who underſtands theſe principles. Bad venting of chimneys is chiefly of two kinds, proceeding from different cauſes: 1*ſt*, A ſlow but conſtant emiſſion of ſmoke into the room. 2*d*, Violent puffs of ſmoke, now and then only, and when particular winds blow, the vent at other times carrying up the ſmoke well enough. The cauſes of the firſt are, 1, Obſtruction or narrowneſs of the vent; 2, Too perfect cloſeneſs of the room or houſe; 3, Too great wideneſs of

the vent below, which allows too much air to get in above the fire, and thus be leſs heated; or, 4. Its being ſeldom warmed by fire.

The cure of narrow and obſtructed vents is obvious. Over-cloſeneſs of the room or houſe, is cured by opening the windows or doors, or making other openings at which the air may enter. A hole above the door of the room is often effectual: Sometimes, however, it is not, namely, when there are well-going vents up ſtairs. In ſuch caſes, the air of the room is diſpoſed to go up ſtairs to ſupply them. You will frequently obſerve that a candle held near the top of the open door indicates a current outwards, while one held near the bottom always ſhews a current into the room. The moſt generally effectual method that I know, is to put a ſlip of wood into the ſole of the window, which will keep the ſaſh from ſhutting cloſe down, but does not allow any air to enter at that place. The air enters by the paſſage which is always left between the upper and under ſaſhes: But it comes in ſuch a manner as not even to diſturb the flame of a candle held cloſe by it. The air flies upwards along the glaſs, ſtrikes againſt the ſoffit of the window, and thence it ſpreads into the room without offending any perſon. In ſome houſes, air comes down the vents in which there are no fires, or in which the fires are weak. Hence we are troubled by our neighbour's ſmoke. Too great wideneſs of the vent is cured by filling up a part of it at each ſide of the grate.

The cauſe of the ſecond kind of bad venting is various, and ariſes from ſituation: It can only be cured by what will increaſe the current up the vent. Contracting the vent below has ſome effect, by obliging the air of the room to come nearer to the fuel, by which it is more heated and expanded, and is, therefore, driven up the vent with greater force But it is more certainly and completely remedied by ſomething being done above.

A certain degree of this bad venting is ſometimes produced by the bad ſituation of she fire-place, and door of the room, with reſpect to

one another. In ſome caſes it is cured by admitting air into the room at ſome particular place or ſide of it, generally that which correſponds to the wind which occaſions the puffs. Sometimes, however, it is almoſt incurable; as, when the wind beats on a higher houſe to leeward. This condenſes the air, and it endeavours to eſcape along any protected route; therefore down the chimney, and from thence out by the doors and windows.

After having thus formed a clear idea of the ventilation of a furnace, it will be eaſy to underſtand the methods of regulating the heats. If we want it very moderate, we diminiſh the ſupply of air, by diminiſhing the aperture either above or below. This is called Damping. It is beſt done by damping below, viz. by cloſing the door of the aſh-pit, and admitting the air through holes proportioned to one another, according to ſome convenient ſeries, which ſhall put it in our power to admit all the intermediate proportions of air. But, in order to have holes, the apertures of which are ſo proportioned, we muſt not make the diameters of them in theſe proportions. This would give apertures widely different from what we want. The areas or apertures of circles are as the ſquares of their diameters. Therefore, having determined on the proportion of the different holes, we muſt make their diameters in the proportion of the ſquare roots of thoſe numbers. I always employ a ſeries of holes in the continual proportion 1, 2, 4, 8, &c. and by uſing more or fewer of them together, any quantity of area can be employed. The conſtruction of this ſeries is exceedingly ſimple. (Fig. 7.) Let the lines AB and AC contain an angle BAC, of 45 degrees. Make AD the ſmalleſt intended diameter. Draw, in ſucceſſion, DE, EF, FG, GH, HI, IK, &c. alternately perpendicular to AB and AC. Then the area of the hole whoſe diameter is AD being 1, the lines AD, AE, AF, AG, AH, AI, AK, will be the diameters of holes, whoſe areas are 1, 2, 4, 8, 16, 32, 64, *(Euclid*, 1. 47.)

When the furnace is very air-tight in the doors and all the joints,

and the fuel of an uniform good kind, the conſumption of fuel will be perfectly uniform, and in proportion to the area of the open holes. In the ſame proportion, as far as we can judge, will be the heat that is kept up. But, if the vent of our furnace have a connection with the chimney of the room, it may be greatly affected by its irregularities. It ſhould, therefore, be unconnected with it, ſtanding open under it.

If we want to raiſe a greater heat, we muſt open more paſſages below the fire-place, or add a tube to the vent, to increaſe its draught. We muſt never admit the air into the furnace above the fuel. This, by paſſing between the fuel and the ſubject, will abate the heat which it receives.

Sometimes the furnace is ſet within a tall dome. By warming the air within this dome, while the furnace receives its air from below the dome, or independent of it, and the dome is ſhut on all ſides, and open only at the top, it is evident that the levity of the air in the dome muſt aſſiſt the levity of the air in the furnace, and thus increaſe the draught and the heat. This will not be the caſe, if the furnace receives its air by its aſh-pit, from the dome, nor if there be any openings in the ſides of the dome.

We ſhall now take a view of the variety that is found among chemical furnaces, or at leaſt point out the moſt uſeful and remarkable kinds. And, as the veſſels were diſtinguiſhed into three diviſions, ſo the furnaces may, in the ſame manner, be brought under three ſimilar heads, or divided into furnaces for digeſtions, fuſions, and evaporations.

Firſt, therefore, for digeſtions, we may occaſionally employ ſeveral different furnaces. But thoſe which have been particularly contrived for this purpoſe, are the ATHANOR, with its ſand heats, and the lamp, and LAMP-FURNACE. The athanor, or perpetual furnace, conſiſts of a tower, round or ſquare, having below, an aſh-pit, grate, and fire-place. The vent goes off laterally immediately above the grate, and is firſt conducted horizontally, paſſing under a row of veſſels, or baths, for

digeſtions, and then terminates in an upright chimney. A little fire is kindled on the grate, and the top of the tower being ſhut, the hot air paſſes along the horizontal opening, goes up the chimney, rarefies its air, and ſoon produces a current. The cover is now taken off from the tower, and this is ſpeedily filled to the top with charcoal, and then carefully ſhut up above. From this conſtruction, it is plain that no fuel can burn for want of air, but what lies immediately on the grate. As it waſtes, the charcoal above it ſubſides, comes into the fire-place, where it burns in its turn, and the heat is kept up as long as it laſts; and we can at any time lift off the cover, and ſpeedily throw in more charcoal. The lamp, and its furnace, are often uſed for diſtillations, and ſhall therefore be deſcribed afterwards.

The ſecond, the MELTING, or WIND-FURNACE, is among the moſt ſimple. When complete, it has all the parts that belong to furnaces in general, and no more. The beſt, for the experiments of the philoſophical chemiſt, are made of plate-iron, well rivetted, and lined with luting. Much has been written about the moſt effectual forms for producing intenſe heats, in ſuch ſmall furnaces. They are gravely recommended to be elliptical, having one focus about the bottom of the crucible, and the other near its top; but theſe are fancies, no way connected with the nature of heat produced by fuel. A great deal of trouble may be ſaved, by quitting theſe fancied forms, and conſtructing this furnace of a ſhape that is the moſt eaſily executed with exactneſs. The drawing here given you, is a ſection of a ſmall air-furnace, and I have found its form and proportions as convenient as any.

The aſh-pit A B (fig. 8.) is of plate-iron, cylindrical, and having on one ſide a door D, a little leſs incurvated than the cylinder, ſo that its latch bends it to the ſides of the aperture, making it ſhut and keep very cloſe. Oppoſite to the door is a regiſter-plate, with a ſeries of holes, each furniſhed with a braſs ſtopper, nicely fitted to it. On the top of the aſh-pit is a flat ring, which carries the grate L K. This being made a little taper, fits exactly into the ring. B E is the body

of the furnace, ſet on the ring of the aſh-pit, and the joint made nearly air-tight, by ſprinkling the ſurface of the ring with fine ſand, or powdered chalk. The body is a little wider above than below, and is cloſe all round. On its top ſtands the dome E F, of a conical form, this joint being alſo ſecured with ſand or chalk. On one ſide of the dome is a large hole G, for ſupplying fuel, and inſpecting the crucible. This is ſtopped with a plug of fire-clay. Two handles muſt be fixed to the dome for lifting it. It terminates above in a wide tube or vent F H, to which additions may be made, when we would increaſe the heat. The crucible is to be ſet on a brick pedeſtal of ſuch a height that it may be nearly in the middle of the body, and be equally ſurrounded with fuel in every direction. It ſhould be rather above the middle of the body, becauſe the hot air is all driven upwards, and it receives little heat from the fuel above it. The taper form of the body cauſes the fuel to collect round it below as it conſumes. The ribs of this, and indeed of all furnace-grates, ſhould be triangular, the narroweſt ſide uppermoſt. This does not allow the cinders to choke the bars, and it gives a ſteadier poſition to the pedeſtal.

If the height B E be 12 inches, the external bottom diameter ſhould be 10 or 11, and the upper one 16. The height of the dome E F ſhould be eight inches. Larger furnaces may be made in the ſame proportion.

The whole muſt be lined with a proper lute, (to be deſcribed afterwards) to the thickneſs of two inches. This is made to adhere by means of a great number of nails rivetted to the external plate, having their points curled like ſo many hooks.

Such, therefore, is the furnace more eſpecially called the melting, or air-furnace, or wind-furnace. The current of air neceſſary to animate the inflammation to that degree of violence which is often required, is obtained merely by the height of the vent. But there are ſome caſes now and then, when, in chemical experiments, we would deſire a rapidity of inflammation, and conſequently an intenſity

of heat, even beyond what this ſtructure can eaſily be made to produce; and this is the caſe ſtill more in many chemical arts, which employ furnaces of ſuch a very large ſize, that to give them a ſufficient height of vent for this purpoſe, would be very inconvenient and even impracticable. In ſuch caſes, we have recourſe to mechanical, or other means of impelling the air through the fuel with ſufficient velocity. Such are bellows of various conſtruction. When theſe machines are of conſiderable ſize, they are generally wrought by the force of water. But there is another way of employing the force of falling water, to impel air into furnaces or fires, which is much more ſimple, and is ſaid to produce as great, if not a greater effect. This is by means of the WATER-BLAST, which Mr. Lewis deſcribes in his *Commercium Philoſophico-Technicum*. The ÆOLIPILE may alſo be conſidered as another way of employing water to drive air into fuel, but in a very different manner. The æolipile is merely a globular veſſel, having a ſmall pipe coming out from it, ſomething like the pipe of a ſtill, but terminating in a very fine bore. Water is made to boil violently in it by ſetting it on a chaffer of glowing coals. A ſtrong blaſt of vapour flows from the pipe. This is directed into the midſt of the fuel, which it animates in the ſame manner as bellows would do; chiefly, I believe, by means of the great quantity of air which it drags along with it from all ſides.

After the melting furnace, and other inſtruments for violent melting heats, the next we ſhall deſcribe, as being alſo often employed in melting certain ſubſtances, though not with ſo violent a heat, is called the ASSAY-FURNACE, ſo called, as it is employed chiefly in the examination of ores. This is not ſo ſimple, by much, as the melting furnace. The operations to which it is adapted require that the ſubject and veſſels be prevented from being in contact with the fuel; that on ſome occaſions, it be expoſed to a gentle current of air which has not paſſed through the fuel; and that the operator have free acceſs to ſee how the changes he propoſes to produce are going on. It has there-

fore an aſh-pit and a door like the melting furnace, and a fire-place and door, with a grate in the bottom, through which the aſhes fall into the aſh-pit. This furnace is not round, but ſquare; and in the middle of the fire-place is fixed a little arched oven, called a MUFFLE, of an oblong form, open before, but ſhut at the far end. The open end of this oven is made to fit exactly the fire-place door, ſo as to appear but one opening; and in looking in, we ſee nothing but the inſide of the muffle. The two ſides of this muffle are pierced with ſeveral openings like arches ſtanding on its floor. When the fire-place door is opened, the freſh air ruſhes in, in order to run up through theſe openings, and thus it plays on the ſubjects expoſed to the violent heat, and burns them completely. This air therefore muſt be freſh. Above, there is a dome or covering, and a tube.

The effect of this furnace is to produce a conſiderable degree of heat under the muffle, and a conſtant ſtream of air through it; and if we want a ſtronger heat, and air is not neceſſary, we cloſe the door of the fire-place altogether.

In uſing the furnaces hitherto deſcribed, the beſt fuel is charcoal, either of wood or of foſſil coal. But I obſerved formerly, that when violent heats are required, and great quantities of matter are to be lighted, it is better to uſe flaming fuel, the flame of which, when well mixed and agitated with air, is capable of giving a moſt intenſe heat. As an illuſtration and proof of this laſt aſſertion, I ſhall ſhew the effects of mixing air with the flame of a candle or lamp by means of the blow-pipe. This little inſtrument is uſed in ſmall works on glaſs, as thermometers, &c. and in ſoldering metals; it is uſeful too to the chemiſt ſometimes in adjuſting veſſels; but, beſides, it is alſo very often uſed as a very quick and ready method of exciting very intenſe degrees of heat, when he wiſhes to examine its effects on a ſmall quantity of matter, ſuch as ores, placed upon a bit of charcoal.

The blow-pipe was recommended by Cramer in his *Ars Docimaſtica*; but the Swediſh chemiſts were the firſt who made much uſe of

it in examining mineral ſubſtances; and one of them, Engeſtroem, publiſhed, at the end of the Tranſlation of Cronſtedt's Mineralogy into Engliſh, an account of the moſt convenient manner of uſing it, in which he made ſome improvements. It is now an indiſpenſable inſtrument for every mineralogiſt, and various contrivances have been introduced for accommodating it for an imitation of almoſt every metallurgical operation. Engeſtroem deſcribes a box, containing the blow-pipe and other implements, which he calls the POCKET LABORATORY. The advantages of the blow-pipe are,—it ſaves a great deal of time and expence, and it enables us to make a multitude of experiments with a ſmall quantity of the materials. The blow-pipe is only fit for heating a ſmall quantity of matter, as the bulk of half a pea, or leſs. Were we to apply it to larger maſſes, and to uſe a wider pipe and much larger flame, it would not produce the effect deſired; the air would paſs through the flame without being well mixed with it, and would ſtrike the matter we meant to heat, in ſuch a manner as to cool it, or diminiſh the heat otherwiſe produced.

Thoſe who make thermometers and other ſmall inſtruments of glaſs with it, often blow it with bellows, but the mouth is moſt ready and moſt under command.

Thus we ſee that flame, mixed and agitated with air, produces a moſt violent heat. When therefore we want an intenſe heat, and a great quantity of it, it is beſt to uſe flame. And there are furnaces particularly adapted to the uſe of flaming fuel.

Such is, firſt, the REVERBERATORY, called by the artiſts who uſe it, the AIR-FURNACE. It is uſed for melting of iron, and for ſmelting the ores of copper and lead, to extract thoſe metals, and for other purpoſes.

It has an aſh-pit, grate, and fire-place, like other furnaces. The fire-place door is large, and ſloping downwards, that the raw fuel may more eaſily be thrown in in abundance, and gather itſelf together on the grate. But the flame produced is not allowed to mount ſtraight upwards, but is diverted laterally, by the low arch, into a ſort of oven.

The floor of this oven is made conſiderably hollow in the middle, and the matters to be treated are laid there. Oppoſite to the entry into this oven from the fire-place, is another opening into the chimney or vent. The effect of this conſtruction is plain. The tall vent occaſions a current of air through the whole furnace, from the aſh-pit door to the top of the chimney. This blows up the fire, and this, by heating the air, increaſes the current, and the fire ſoon becomes furious. A volume of flame riſes from the raw fuel; this is hurried into the lateral paſſage mixed with air; and the form of that paſſage cauſes the ſtream of flame to dart violently down on the materials lying in the bed of the laboratory (ſo is the oven generally named). The low roof of this place, and ſome inequalities artfully made in it on purpoſe, confine the ſtream of flame to the ſubject, and, by toſſing it into violent eddies on its ſurface, mixes the air moſt intimately with every part of the flame, and thus completely conſumes the whole combuſtible matter of the fuel, ſo that, in a good reverberatory, not a particle of flame gets as far as the bottom of the vent. In the *Journal de Phyſique*, 1776, there is a deſcription of a Reverberatory Furnace, which ſeems very ingeniouſly fitted for a general experimental furnace.

Thus a moſt intenſe heat is produced on the matter lying in the bed of the reverberatory. There are openings made in the ſide of it, juſt where the matter lies. Here the workmen, from time to time, open the door, and put in their tongs and rakes, and other inſtruments, to ſtir and manage the ſtuff according to their views. Frequently, too, the nozzle of a pair of bellows is directed on the ſurface of the ſtuff, to burn it ſtill more completely, and, blow the liquid ſlag and the droſs from off its ſurface to the other ſide, where a way is made for its getting out.

Another furnace, in which the flame of fuel is employed, is the Glass Furnace. The ſpecialities of this depend on this circumſtance, that it is neceſſary for the workmen to have free acceſs to the matter,

and, at the ſame time, to have a violent degree of heat kept up. An ordinary furnace, therefore, with a high vent to produce a draught, and a door, or opening at the ſide, will not anſwer; becauſe the cold air would be driven in at this opening, and run up the vent, and thus cool both the fire-place and vent. A glaſs houſe, therefore, is ſo conſtructed, that there is a high building, of a conical, or other form, over the furnace, which ſerves as a vent to increaſe the current of air, and is ſo wide all round, that there is room for the workmen. The air within this building is heated conſiderably by the furnace ſtanding in the middle of it, and matters are ſo diſpoſed, that the current of air thus produced is made to paſs chiefly through the furnace, by the aſh-pit, which is under the floor of the furnace and DOME, and has a free communication with the external air, but none with the dome. No more is admitted to come in at the doors of the building than what is merely neceſſary to render the heat tolerable to the workmen, and to allow them breath. Fig. 9. will illuſtrate this deſcription. A is the grate for the fuel, B, B, B, the pots, D the holes where the workmen dip their rods into the pots, C the vault of the furnace, and E the aſh-pit, communicating with the air without doors. The whole is ſet in the middle of a vaſt dome, F. The air coming from the aſh-pit through the grate and fuel, aſcends through the working holes D into the dome, and thus makes a great column of warm and light air, which riſes through the opening of the dome with great force. Some of the holes G open into ovens, in which the materials for glaſs are roaſted, to drive off their volatile parts. Thus it appears, that the working holes are the immediate chimneys or vents of the glaſs furnace. We cannot otherwiſe obtain the heat that is neceſſary. For if there were chimneys, as in other furnaces, the air, ruſhing in at the work holes, would make it cold in the place where we want the greateſt heat. The workmen are therefore expoſed to almoſt intolerable heat. But ſuch heat is not always neceſſary. Chiefly while the glaſs is preparing, and in a ſtate like ebullition, much volatile matter is eſcaping

from it. Melted glaſs being ſo viſcid, it muſt be made to flow as thin as poſſible, that theſe volatile matters may make their way up through it. When the glaſs is ready for working, they open ſome of the doors of the dome. This admits the freſh air, in which the men can breathe and work. It abates the heat of the riſing column of air in the dome. This abates its aſcenſional force, and, conſequently, the heat of the furnace. This more moderate temperature is called the *ſettling heat*, the frothing glaſs being now quiet. After this they increaſe the heat a little, to bring the glaſs into a proper ſtate for lifting. This they call the *working heat*.

This, therefore, is the furnace for melting together the materials of glaſs in large quantities. If our purpoſe be only to make ſmall quantities, or compoſitions of different kinds of coloured glaſſes, or imitations of gems, or experiments of that kind, we may conſtruct ſuch a furnace, as is deſcribed by Cramer, at the end of his Art of Eſſaying, in which he ſays that a great number of experiments may be made at the ſame time, with great eaſe and convenience.

It remains now only to conſider the furnaces for vaporiſation.

The firſt of theſe I ſhall mention is what Macquer calls the reverberatory, or for performing diſtillations that require the ſtrongeſt heat, in which the veſſels employed, commonly of earthen ware, are expoſed to the contact of the fuel; on account of which it is commonly called the furnace for diſtilling with naked fire. This furnace is deſcribed by Macquer. It has nothing very different from the melting furnace, only there are two openings at the fire-place; the one is a door for the fuel; the other is circular, between the dome and the body, intended for allowing the neck of an earthen retort to point out. This hole is formed by making a ſemi-circular notch in the upper part of the body, and another in the lower part of the dome. When theſe are ſet together, the neck of the retort finds room between them. If this hole be ſhut by an earthen plug, the furnace is the common melting furnace. Likewiſe two iron bars muſt be placed acroſs

the middle of the fire-place, to ſupport the bottom of the retort; or, inſtead of theſe bars, we may have a pedeſtal. When this veſſel is put in, the opening round its neck ſhould be filled up with luting. It ſhould have a high vent and dampers.

The ſecond furnace for vaporiſation is the furnace of the common ſtill, having a vent that iſſues out at one ſide.

3d, Other diſtilling furnaces are chiefly fitted for retorts, or veſſels of glaſs, and are neceſſary for uſing theſe veſſels, applying the heat ſlowly and equally to the whole ſurface. The particulars in which they differ from others are only contrivances for obtaining this end. Therefore, their upper part immediately over the fuel is covered by a hemiſpherical pot of caſt iron of ſome thickneſs, in the middle of which the veſſel is placed and ſurrounded with water, aſhes, ſand, or other matters. Thus, the pot having ſome thickneſs and ſolidity, it heats ſlowly and equally, and the matters with which the veſſel is ſurrounded tranſmit this ſtill more ſlowly; and the whole, being once heated, though the fire may be a little unequal, is not ſubject to ſudden variations, which glaſs veſſels are unable to bear. The matters interpoſed between the pot and the glaſs veſſel have got the name of balnea or BATHS. The WATER BATH, *balneum mariæ*; the SAND BATH, *balneum arenæ*. Iron filings are ſometimes uſed; others have propoſed quickſilver, and melted metals, as tin, lead, and biſmuth. Sand is the moſt convenient of any. The older chemiſts frequently uſed no intermedium but air, ſupporting the retort on three bits of chalk, and ſetting over it a little dome of plate iron, notched, to admit the neck. This they called diſtilling *in capellâ vacuâ*. This is a fooliſh method, becauſe, unleſs the ſpace round the neck of the retort be carefully ſhut, which it is very difficult to do, there is a continual current of air circulating round and eſcaping, and the retort is very unequally as well as feebly heated.

A lamp furnace, and lamp, have been ſometimes uſed for diſtillations, and they are, no doubt, extremely neat and convenient, when

a very moderate heat is ſufficient. But ſpirit of wine is the only fuel we can employ. To manage an oil flame requires more attention than a fire of charcoal or coaks.

Some have propoſed a train of furnaces, or ſand heats, for the hot air, or heat of an athanor furnace, to paſs through; but theſe are of little uſe.

A variety of other furnaces are uſed in various chemical arts, as enamelling, ſmelting metals from their ores, and refining and ſeparating them from one another. But a deſcription of thoſe furnaces will not be inſtructive, unleſs in giving a particular account of thoſe arts themſelves, which we cannot now do. I have only given you the chief varieties commonly uſed by ſpeculative chemiſts in the way of inquiry and experiment, explaining, as I went on, their principle and mode of action. The ſame principles affect the performance of all furnaces, and, therefore, this knowledge of them not only enables you to conſtruct ſuch furnaces as I have deſcribed, but alſo to conſtruct furnaces properly accommodated to the circumſtances which occur in the practice of the chemical arts. I deſcribed the glaſs-houſe furnace, becauſe the great dome is a part which does not occur in the furnaces of the philoſophical chemiſt, and yet an intelligent chemiſt may find many caſes where it may be applied to other furnaces employed in a chemical manufacture. It has been ſucceſsfully employed to increaſe the heat in ſmall furnaces for ſmelting iron from the ore, and in very ſmall lime-kilns.

But beſide the contrivance of theſe *particular* furnaces, as they may be called, attempts have been made to contrive what may be called a GENERAL FURNACE, that is, one applicable to all the chemical operations. The attempts of this kind which have been attended with any remarkable degree of ſucceſs, are not many in number.

The firſt, ſo far as I know, which drew any attention, was that of Becher, the famous German chemiſt, He publiſhed a pamphlet, to which he gives the myſterious title of *Tripos Hermeticus Fatidicus;*

the Chemical Fortune-telling Tripod; by means of which, to wit, an adventurer in alchemy was to learn, by trial of his own ſkill, whether he ſhould be ſucceſsful or not. The pamphlet contains the deſcription and drawings of a furnace, conſiſting of four or five different pieces, which can be joined variouſly together, ſo as to adapt the furnace more or leſs completely to the purpoſes of digeſtion, or fuſion, or the eſſaying and refinement of metals, or diſtillation, either with earthen retorts and a ſtrong heat, or the common ſtill, or in glaſs veſſels ſet in warm ſand, or in hot water. You may ſee the drawings and deſcription of this furnace at the end of Dr. Schaw's Engliſh Tranſlation of Boerhaave's Chemiſtry, Dr. Schaw having copied it from Becher without mentioning his name. The different parts of this furnace are made of earthen ware, or a ſort of clay, ſuch as fire-bricks are made of; but our potters here are not accuſtomed to ſuch work.

This furnace is conſtructed with much ingenuity and judgment, and does honour to this early chemiſt. When accurately made, ſo that the joinings of its different parts can be made tight with a little chalk, I believe that it may be eaſily managed in all its functions. But ſuch earthen ware is very ſubject to crack. This may be ſecured by hooping. But frequent placing and ſhifting the pieces on each the other, ſoon chips the edges; the joinings grow open, and then there is no regularity in the heats, and veins of cold air get in and crack our veſſels. A pottery, alſo, which is ſufficiently ſtrong for the furnace, will tranſmit the heat ſo much as to become extremely diſagreeable, and it becomes very troubleſome to keep receivers cool enough for condenſing the vapours.

Another contrivance, by which we can more readily provide ourſelves in this country with a general furnace, or a ſet of furnaces for chemical experiments, is deſcribed by Mr. Lewis in his *Commercium Philoſophico-Technicum*. The author of this contrivance was a goldſmith. Being daily accuſtomed to uſe crucibles, it occurred to him

that thoſe made of black-lead, ſome of which are of a large ſize, might eaſily be converted into furnaces, which would ſerve for chemical experiments on ſmall quantities of matter. The black-lead crucibles have the advantage of being ſo ſoft, that it is eaſy to bore holes through them, or to cut doors in the ſides of them, or to cut or grind off any part of them to adapt them to ſuch a purpoſe. And they alſo bear unequal heat, or ſudden changes, with far leſs danger of cracking or ſplitting than earthen ware does. Mr. Lewis, therefore, teaches how the largeſt of them may be converted into furnaces for different purpoſes, either ſingly in ſome caſes, or by joining two of them together in others; and, in others, by additional tubes, or other pieces which he adapts to them. As he gives exact deſcriptions and delineations of all this, I need not be more particular here, but refer you to his work. The contrivance is certainly ingenious, and affords a ſhort and ready method for providing ſmall furnaces. But they have their faults. They are not ſufficiently durable, black-lead being conſumed or deſtroyed by the repeated action of violent heat. Neither do they contain enough of fuel; nor can they be regulated with ſufficient exactneſs.

I have therefore endeavoured to contrive a general furnace for myſelf, which might be free from thoſe imperfections, and I ſhall now deſcribe it. It has the advantage of being far more ſimple than thoſe I have already mentioned.

Plate-iron is by far the beſt material for the outſide of an experimental furnace: but, as its metal communicates heat very faſt, this muſt be cut off by a proper lute lining. I have ſo far ſucceeded in this reſpect, that my furnace, though only two inches thick in the middle, will not ſcorch paper applied to its outſide, when it is melting iron within. I make it of the ſimpleſt rectilineal ſhapes, becauſe workmen find great difficulty in executing curved and uncommon forms; and not one of a ſcore of them will do it with accuracy. Indeed, thoſe highly praiſed forms ſeem to me of very little importance in moſt

caſes. The only caſe in which I find it of value, is in the laboratory of a reverberatory furnace, where ſomething may be done in this way for mixing the hot air and flame, and making it play with force on the ſubject to be treated.

The body or fire-place is the only part of this furnace that requires deſcription; the aſh-pit, with its door and regiſters and grate, being conſtructed as in any other furnace. It will be eaſily underſtood by conſidering the ſection repreſented in fig. 10.

The baſe, repreſented by the dotted line A B C, and the top K L M, are oval plates of iron, the longer diameter A C, being to the ſhorter as three to two nearly. The baſe and top are equal, ſo that the ſides K A, M C, are upright, the whole body forming an oval cylinder. D E F is half of the hole in the bottom, which is occupied by the grate fixed on the top of the aſh-pit. G H I is half of the mouth of the furnace, which receives a ſtill, or a ſand-pot for diſtillation, with a retort. This is a little nearer to the front K of the top, than the grate hole is to the front A of the bottom, ſo that the luting is thicker below than above. Near the back M of the furnace, is a ſmaller hole P for the vent. The luting at Q and R is ſo formed, that the cavity of the furnace does not greatly differ from a cylinder, except in ſo far as the vent P O does not communicate with it abruptly, but is gradually curved downwards, as repreſented in the figure, making the middle of the cavity more roomy backwards; by which means it contains a greater quantity of fuel. S is the ſection of the luting, which forms a ſort of an arch, or bridge, contracting the entry of the vent. An iron pipe is ſet on at P, to increaſe the draught of the chimney. The fuel is put into the furnace by the aperture P, and the ſloping form of the cavity cauſes it to diſtribute itſelf pretty uniformly.

When the furnace is uſed for ſmelting, the crucible is ſet on a pedeſtal ſtanding on the grate, and the fuel is placed round it with great eaſe, the mouth of the furnace being open. This is then ſhut up by a ſtopper made on purpoſe, or by a flat fire-tile ſimply laid on it.

When we would diſtil with a naked fire, the retort has its bulb reſting on a ring which hangs on the mouth of the furnace by three hooks, and the neck of the retort lies over the front of the furnace. The ſpace round the retort at the mouth of the furnace is cloſed, as much as is neceſſary, by two or three pieces of tile, ſhaped ſo as nearly to fit the bulb of the retort when they are laid on the mouth of the furnace. A quantity of light aſhes are now to be laid on theſe tiles, and heaped up ſo as to cover the bulb and part of the neck of the retort. I find that this produces a very gradual diminution of the heat as it recedes from the fuel, and is leſs liable to crack the retort, by inequality of heat, than any contrivance I know. Scarcely any proceſs occurs which this furnace does not anſwer with great eaſe.

We have now ſaid enough on the ſubject of furnaces. There is only one article more of the apparatus which remains to be mentioned,—I mean the LUTES.

In uſing the furnaces which I have deſcribed as moſt convenient for experimental chemiſtry, (I mean thoſe made of plate iron) it is neceſſary that the iron be defended from the heat by lining or lute, as we called it, on the inſide; and ſuch lutes are neceſſary in other occaſions in chemiſtry; as when we have occaſion to cloſe the joining of veſſels with one another, or to give a coating to retorts, or even to crucibles, which is ſometimes done. The materials employed for theſe purpoſes have their general denomination from clay, of which ſome of the moſt uſeful are partly compoſed, though there are ſome that do not contain any of it. They may be divided into ſuch as contain animal or vegetable matter of the glutinous or adheſive kind, and ſuch as are compoſed only of earthy ſubſtances. The firſt are uſed for cloſing the joining, of veſſels, when the heat we mean to apply is not to be ſtrong, nor the vapours to be produced corroſive. The ſecond ſerve for the lining of furnaces, or for cloſing the joinings of veſſels, in operations in which the vapours are very corroſive, or in

which a ſtrong heat muſt be employed, which would ſcorch, or burn and deſtroy any animal or vegetable glutinous matter.

The joinings of veſſels with one another, which we have the moſt frequent occaſion to cloſe up by means of lutes, are thoſe of retorts with receivers. And we may remark, in the firſt place, with regard to theſe, that there are not many operations in which it is neceſſary to make the joining perfectly cloſe, except when the receiver is provided with an air-pipe. On the contrary, it is dangerous, on account of the air, which muſt be allowed to eſcape in ſome manner. Therefore we are not anxious to contrive the moſt cloſe and compact. They are ſufficient, and better, if they be moderately ſo, and, in ſome caſes, when we think the lute too cloſe, we even obviate it by a pin-hole. The animal and vegetable lutes, employed in this way, are glue and chalk mixed in thin paſte, and ſpread on ſlips of paper, or gum Arabic and chalk, uſed in the ſame manner, or flour and water, or a bladder, or lintſeed meal, or fat lute. Mr. Lavoiſier recommends, for joinings which we deſire to be air-tight, but which are not to be expoſed to heat, the following: To 16 ounces of bees wax add 1½ or 2 of turpentine, and keep it for uſe. When uſed, ſoften, and make it tough, by warming and working between the fingers; then put it on the joint in little rolls, and make it cloſe; and, laſtly, cover it with ſlips of wet bladder laced with pack thread. But, if the joint is liable to be warmed or heated during the operation, you muſt take fat lute. This is made of raw pipe clay and lintſeed oil, beaten together very hard, to the conſiſtence of a ſtiff adheſive paſte.

Of the ſecond kind of lutes, called the Fire-Lutes, a great variety have been propoſed, and ſome of them compoſitions of many ingredients, ſome of which I have tried, but find none equal or ſuperior to clay and ſand, viz. ſand 3, or 4, or 5, or 6, to clay 1. Theſe are for luting veſſels together, and for coating. But in lining furnaces, I uſe a double lining; *1ſt*, a charcoal lute; *2dly*, a fire-lute.

It may appear paradoxical to ſpeak of a charcoal lute for lining a furnace. But I find that a layer of powdered charcoal, beaten up, or kneaded, with as little water as will give its particles adheſion enough to attach itſelf to the metal ſides of the furnace, by means of cautious beating, forms a firm ſtratum, which is the moſt imperfect conductor of heat of all that I have tried. When this layer of charcoal is defended from the action of the air by a layer of fire-lute, compoſed of one part of fine clay, and three or four parts of ſand, carefully put on, and conſolidated by gently beating it from day to day, till it no longer receives an impreſſion from the mallet, it will laſt as long as any part of the furnace. Its durability will be greatly improved, without much change in its conducting power, by uſing, inſtead of pure water, water made muddy by about 1-20th of pipe-clay. If finely powdered charcoal be kneaded with 1-5th of pipe-clay, it may be kneaded and formed into any ſhape, and will be ſo impervious to heat, that a bit of it may be held in the fingers, within an inch of where it is red hot. Such a compoſition is, therefore, very proper for the doors of furnaces, and for ſtopples for ſuch apertures as muſt be frequently opened and ſhut.

Thus have I taken a ſurvey of the apparatus, and have thrown in ſuch remarks upon the manner of uſing ſeveral parts of it, as could be delivered in a general way. It is not neceſſary at preſent, and it would be tireſome, to enter into further details. You will have the opportunity of obſerving the conduct of a variety of operations which will be performed during the reſt of this courſe; and ſuch as deſire to make this part of chemical knowledge an object of their attention, will find inſtructions for performing every particular proceſs in the numerous books upon the Practice of Chemiſtry—Macquer, Boerhaave, Lemery, Wilſon, &c.

We have now gone over all the articles that can with propriety be called general, affecting every branch of chemical inquiry, or requiring to be well underſtood, in order that we may proſecute this in-

quiry with ſkill and ſucceſs. Our next occupation will be to examine the chemical properties, and mutual relations, of that immenſe variety of bodies which nature preſents to our view. The natural philoſopher, or mechanician, looks at the ſame object that we do, but with very different purpoſes. He only inquires whether the body be moveable, and what force will move it: If it will move altogether, when touched in any one part, or if it will ſeparate like ſand or water. He inquires whether it be hard, ſoft, liquid, or elaſtic; and the intereſting properties which he finds in one, he finds in all; and they are not only general, but obvious. To the mechanician, a pound of gold, and a pound of free-ſtone, are nearly the ſame.

The natural hiſtorian ſees much greater varieties in the objects around him; but, if he ſtrictly keeps within the pale of his ſcience, he ſees only the ſurface; he attends to the colour, the ſhape, the configuration, the ſtructure, and perhaps the ſtrength of the connection of the different parts.

To the mechanician and the natural hiſtorian, gunpowder is little more than an aſſemblage of black grains, of a certain weight and firmneſs. What a different and intereſting object is it to the chemiſt, when he examines it by his inſtruments, mixture and heat! He can get aquafortis and potaſh out of it, and brimſtone, which all the powers of mechaniſm cannot ſeparate from it, and which the natural hiſtorian cannot diſcover in it. If the chemiſt apply his other great inſtrument of reſearch to it, what wonderful effects and productions will he now behold! A force appears, of which it gave not the leaſt indication, and which exceeds all the powers of machinery. Scorching heat, dazzling light, and deafening noiſe, ſtrike him at once; his gunpowder is diſſipated, not into its ingredients, for neither aquafortis, potaſh, nor brimſtone, are to be ſeen; and, inſtead of a cubic inch of gunpowder, he finds a vaſt volume of elaſtic air.

Thus do thoſe two inſtruments of knowledge, thoſe two ladders of ſcience, give us acceſs to the moſt remarkable, and the moſt intereſt-

ing properties and relations of bodies. Every ſtep we take gives us a new and a uſeful acquaintance. If, therefore, you have found intereſt and inſtruction in what has hitherto occupied your attention in this place, I think that I can aſſure you of ſtill more in the application of the knowledge you have acquired.

THE CHEMICAL HISTORY OF BODIES.

INTRODUCTION.

HAVING acquired ſome knowledge of the general effects of heat and mixture, the great chemical agents in nature, you are enabled to underſtand the more particular doctrines of chemiſtry. Theſe comprehend the deſcription of all the great claſſes of natural ſubſtances, and the peculiarities which are obſerved in the effects of heat and mixture upon each; which peculiarities ſerve as diſtinguiſhing characters of each, aſcertaining what may be called their chemical nature, qualities, and powers. It is thus that we are able to diſtinguiſh gold from lead. The firſt bears the moſt intenſe heat of our furnaces, and the action of air, while thus ſubjected to heat, without any change in its qualities; whereas the ſame treatment will quickly change the lead into drofs or glaſs. Theſe peculiarities, being conſtant and uniform, give us certain marks of diſtinction, and alſo enable us to employ both ſubſtances for very intereſting purpoſes.

If the peculiar effects of heat and mixture on an individual body be altogether peculiar to itſelf, the ſtudy of them would be endleſs. But there are certain claſſes of natural ſubſtances, on which theſe agents produce effects ſo ſimilar, that one of them may be taken as the repreſentative

of all the reſt. Philoſophers have taken much pains to claſs the ſubſtances that we ſee around us, by means of thoſe reſemblances in the effects of heat and mixture that have been obſerved, and have thus obtained a chemical arrangement of natural ſubſtances, which makes the ſtudy of their properties much more expeditious and comprehenſive.

In thinking on a proper method, the one which occurs the moſt readily, and which appears at firſt ſight the moſt proper, is to begin with an account of the elements, or elementary kinds of matter, of which all things are compoſed; and, after explaining the powers and properties of thoſe elementary ſubſtances, to ſhew how, by their various combinations with one another, they give riſe to the various productions of nature. And, that there is ſome foundation for thinking of ſuch an order or ſyſtem, is ſufficiently plain. It is evident that there are in reality certain elements, or elementary ſubſtances, which are few in number, and of ſuch a nature as to be exempted from decay or change, but capable of being variouſly compounded together, and ſeparated again, ſo as to give origin to the productions which we ſee daily ariſe, and to the perpetual changes of one form or ſtate of matter into another.

This is evident from the conſtant regularity and general uniformity of the productions of nature. There is a certain ſucceſſion and rotation of forms which the ſame matter undergoes, and through which it has repeatedly paſſed with regularity, from the beginning, or from the earlieſt account of time of which we have any knowledge. At leaſt, any examples of irregularity that occur are ſo inconſiderable in number, that they may be overlooked. The water ariſes in vapours from the ocean, chiefly to form the clouds; theſe again deſcend upon the dry land in rain, which, penetrating the ſoil, occaſions and ſupports the vegetation of plants. The greater number of plants ſerve for the nouriſhment of animals, all of which, with the remaining plants, are diſſolved again at laſt, and return to the elementary ſubſtances from

which they were originally compoſed, and from which ſimilar plants and animals are to be produced.

This rotation has continued from the earlieſt accounts of time, in the moſt ſteady and regular manner. The productions of nature have not only ſucceeded one another in the ſame general order, but have been from the beginning, ſo far as our knowledge reaches, invariably the ſame. An oak of the preſent time has the ſame general nature, and the ſame properties, as thoſe of all other oaks that have ever exiſted. We find the ſame texture in its wood and bark; a ſimilar diſpoſition in general in the root and branches; the leaves have the ſame form; the juices the ſame aſtringent power; the fruit is moulded to the ſame form of an acorn, and has the ſame invariable property of never producing any other tree than an oak.

All this ſhews that the elements of bodies are permanent and unchangeable. Had they been liable to any gradual change or decay, the oaks of the preſent times, compoſed of thoſe changed materials, ſhould not be found to have the ſame qualities as the oaks of remote ages; and the order and courſe of nature, as well as the qualities of her productions, would have been undergoing a gradual change, which, ſo far as our knowledge reaches, has not actually taken place.

Such obvious reflections on the courſe of nature have, in the earlieſt ages of philoſophy of which we have any accounts, ſuggeſted the ſuppoſition of a certain number of unchangeable elements, of which it was imagined all things were compoſed, and on the ſucceſſive ſeparations and re-unions of which, depended the decay and reproduction of the objects which nature preſents to our view. This was the meaning of the opinion aſcribed to Democritus, That all things were formed of atoms. And others of the Greeks, in conſequence probably of their intercourſe with the Egyptian philoſophers, began to form and teach a ſyſtem upon this ſubject. They aſſigned four elements, or elementary kinds of matter, viz. fire, or the matter of *fire*, *air*, *water*, and

earth; and by the different combinations of thoſe elements, they ſuppoſed that all the different bodies in nature were formed.

Theſe four ſuppoſed elements were accordingly conſidered, until very lately, as the primary elements of natural bodies; inſomuch, that Mr. Macquer, in his Syſtem of Chemiſtry, thought it proper to begin by treating theſe four kinds of matter as elementary, and endeavoured to explain the nature and qualities of ſome of the productions of nature, which he firſt deſcribes by aſſigning the number of theſe elements which he imagined entered into their compoſition. But it appears from the ſequel, that he was not fortunate, nor ſufficiently conſiderate, in chooſing this method.

In the firſt place. chemiſtry gave, at that time, too little light into this ſubject, to enable him to form a ſyſtem of this kind with any degree of aſſurance. On the contrary, there were many reaſons for doubting, even at that time, that theſe four ſuppoſed elements were in reality ſimple, elementary, and unchangeable ſubſtances; and now, we have direct evidence from experiments, that moſt of them are not.

Atmoſpheric air has been demonſtrated to be a compound maſs of more than two or three different kinds of matter.

Water is alſo believed now to be a compound body.

And of the pureſt and moſt ſimple earthy matter, ſix or more different kinds are now reckoned, which we cannot reduce to greater ſimplicity by our operations.

The truth is, that our knowledge of nature is not yet brought to that ſtate of advancement, which might enable us to follow, with propriety, an order of this kind. We have advanced a conſiderable way in our analyſis of many different ſubſtances; but our knowledge of their compoſition and origin is far too imperfect, to give ſolid foundation for an attempt to arrange them, or to explain their different qualities, by beginning from their ultimate elementary principles. We ſhould certainly be in danger of falling into many errors and miſtakes by ſuch an attempt.

To avoid this, therefore, I do not pretend todetermine what are the ultimate elements of bodies. I content myſelf with diſtinguiſhing and dividing the principal objects of chemiſtry into a number of claſſes, each of which comprehends ſubſtances that bear a remarkable reſemblance or analogy with one another, in their chemical properties, and differ from thoſe comprehended in the other claſſes. And I offer theſe claſſes to your conſideration in what appears to me to be the moſt convenient order;—I mean the order in which the moſt independent, and I may ſay elementary *facts*, are firſt ſtated; and the reſt are deſcribed afterwards, in that ſucceſſion and order in which they can be explained and underſtood, by the knowledge of the facts and doctrines that have been ſtated before them.

An attempt to form claſſes, or aſſemblages, of the objects of chemiſtry, was made long ſince by the elder chemiſts, and their claſſes were three in number. They choſe to give them the title of *kingdoms*. The *firſt* comprehended all animals and animal ſubſtances; the *ſecond* the vegetables; and the *third* foſſils. Theſe aſſemblages may appear at the firſt view to have ſome foundation in reaſon, but they are in fact very improper. They bring together a great number of objects, which have no ſimilarity in their chemical properties, and they ſeparate others, which being ſimilar, or the ſame in a chemical view of them, ought certainly to be aſſembled and conſidered together.

In the arrangement which has prevailed for ſome time paſt among the chemiſts, and which I have found to be the moſt ſuitable to our purpoſe, the objects of chemiſtry are diſtinguiſhed into five claſſes, which are, 1ſt, *The Salts;* 2dly, *The Earths;* 3dly, *The Inflammable Subſtances;* 4thly, *The Metals;* and 5thly, *The Waters*.

Theſe claſſes take in a great part of the materials of which the animal and vegetable ſubſtances are compoſed. But yet there are ſome of theſe materials that do not, on a firſt conſideration, eaſily find a place in any of theſe claſſes. And beſide this, the animal and ve-

getable ſubſtances are compounded in ſuch a particular manner, that they have ſome chemical qualities peculiar to themſelves on that account alone. For theſe reaſons, it is proper and neceſſary to take a chemical view of the vegetable and animal ſubſtances, after the five claſſes already mentioned have been conſidered; and this ſhall accordingly be done in the end of this courſe.

N. B. It may perhaps be thought that we ſhall be guilty of an omiſſion, if we do not conſtitute a claſs of AIRS or GASES, as they are now named. But there is no neceſſity for conſtituting ſuch a claſs, and even the propriety of it may be called in queſtion.

Theſe gaſes are acknowledged to be various kinds of matter, combined with latent heat, or the matter of heat, the *calorique* of the French chemiſts. And why ſhould ſubſtances of ſuch very different natures be aſſembled into one claſs? For this reaſon alone, that they were combined with latent heat, and thereby reduced to a form or condition, to which many other bodies can be reduced by it. We might as well think of claſſifying the objects of chemiſtry into thoſe that are hot and thoſe that are cold, or into thoſe that are ſolid and thoſe that are fluid, which would be very improper, when it is in our power to make the ſame body hot or cold, or ſolid or fluid. It muſt be confeſſed that we have not been able hitherto to deprive ſome of the gaſes of their latent heat, and to reduce them to a condenſed ſtate, without combining them with other matter; but this is certainly in our power with regard to others; and means may poſſibly hereafter be diſcovered for condenſing them all. But whether this ſhould ſucceed or not, there is no reaſon for aſſembling into one claſs ſubſtances, which are, in fact, more diſſimilar in their chemical properties than any ſubſtances that we know, and reſemble each other only in a quality, almoſt purely mechanical (their elaſtic fluidity) merely becauſe they derive this aërial form from heat combined in them.

But, although it appears improper to form ſuch a claſs, I do not mean to leave theſe ſubſtances unnoticed. Very convenient op-

portunities will occur during this courſe for bringing them into view, and I ſhall not fail to make you thoroughly acquainted with them all. *(See Note 26. at the end of the Volume.)*

In proſecution, therefore, of the arrangement that I mentioned as peculiarly convenient, I begin with the conſideration of CLASS FIRST.

CLASS I.

SALTS.

THIS claſs is placed firſt, becauſe the knowledge of their properties, both general and particular, is almoſt indiſpenſably neceſſary for the inveſtigation of the intereſting properties of all other ſubſtances. They ſtand in very intereſting relation to every chemical ſubſtance; and thus are conſidered as the moſt general agents in the way of mixture. This, of itſelf, raiſes a ſurmiſe that they are of a very ſimple nature, and nearly allied to the elements of which all are compoſed. This has been a very current opinion, and has been ſupported by ſpecious arguments. But I apprehend, that before we have gone over the five claſſes of our arrangement, we ſhall find the proofs becoming very weak indeed.

In deſcribing the ſalts, I ſhall firſt enumerate the qualities common to them all, and afterwards take a nearer view of their ſeveral kinds. Moreover, as, in defining chemiſtry, I ſaid that it is the ſtudy of the effects of heat and mixture on all bodies, ſo, in enumerating the chemical qualities of ſalts, and, indeed, of all bodies of whatever claſs, I ſhall firſt deſcribe the effects of heat on them, and afterwards, the conſequences of mixing them with one another, and with other bodies. Moreover, in doing this, I ſubject myſelf to a rule which I ſhall endeavour to follow, with all reaſonable ſtrictneſs, through the whole of this courſe, namely, that while I am treating any particular ſubſtance, or claſs of ſubſtances, in the way of mixture, I ſhall mix them with *thoſe others only* which have been already deſcribed. If

I cannot rigidly abide by this rule on every occaſion, I ſhall be careful to conſider the mixture with ſubſtances which are as far known to you as the argument propoſed by the mixture may require. No quality of the other ſubſtance ſhall be employed in the argument, but ſuch as is known with the fulleſt evidence, and underſtood, as far as I ſhall employ it in argument. The reſult of this caution on my part, will, indeed, be a ſlower progreſs than may be acceptable to ſome impatient minds, but the knowledge which we ſhall acquire will be more confident and ſatisfactory. Sir Iſaac Newton ſaid to a celebrated foreigner, who was complimenting him on his wonderful diſcoveries, and the greatneſs of his genius, which was able to comprehend ſuch vaſt objects, "Indeed, Sir, you are in a miſtake on "both points. The objects are, indeed, vaſt and magnificent, and "therefore I made a choice fortunate for my reputation; but they "are comprehenſible by the moſt ordinary genius, if he will take "my method,—never to hurry. If I have any advantages over "other naturaliſts, it is only in a more patient thinking, in which I "perhaps exceed many of them. To this I am indebted for all my "ſucceſs." Let us imitate the patience of this great man, and hope that we ſhall have ſome portion of his ſucceſs.

In compliance, therefore, with my rule, in the conſideration of the ſaline ſubſtances, I ſhall, at preſent, take notice of ſuch qualities only as they manifeſt in mixture with water, and with one another, deferring the effects of mixing them with earths, metals, and other bodies, till we conſider thoſe claſſes themſelves.

OF SALTS IN GENERAL.

In the definition which is commonly given of a ſalt, an attempt is made to enumerate the qualities by which ſalts are known to agree with one another, and to differ from other claſſes of the objects of chemiſtry.

Thus, the *ſalts*, in general, are ſaid to be *ſubſtances eaſily melted*,

or volatilized by heat, ſoluble in water, not inflammable, and ſapid, or, when applied to the tongue, producing the ſenſation of taſte.

Some objections may be offered to this definition; but it is impoſſible to contrive a definition of them that will be free from all objections; and the ſame difficulty occurs in attempting to define any other aſſemblage or claſs of the productions of nature. There are, in reality, no claſſes in nature diſtinctly defined; the productions of nature are ſo very numerous, and ſo much diverſified, and there is ſuch a gradation throughout the whole, from one to another, that it has been found impoſſible to arrange them into claſſes preciſely limited, and liable to no exceptions.

I ſhall not, therefore, be anxious to give an unexceptionable definition of the ſalts. I hope to give you better information of them, by entering into their hiſtory, and by enumerating their qualities.

The moſt pure ſtate in which we can exhibit the ſalts, or the ſtate under which they are the moſt free from admixture with all other matter that is not ſalt, is the form of a white, hard, brittle maſs, more or leſs tranſparent, and, in ſome caſes, perfectly tranſparent.

The effect of heat on ſalts is firſt to produce fuſion, with an appearance like white wax. All the ſalts can be melted in this manner, but they require very different heats to their fuſion. Moſt of them require different degrees of red heat. When cooled, they return to their former ſtate, and it is only by fuſion that they are brought to that now deſcribed.

Salts can alſo be made to aſſume the form of vapour, by heats under our command; but they differ ſtill more widely with reſpect to the force of heat neceſſary to produce this effect. Some require the moſt violent heats that we can raiſe; others are ſo volatile that they are diſpoſed to aſſume the form of vapour even in the ordinary heats of the air. Their differences in this reſpect ſhall be mentioned when we are deſcribing the different kinds of the ſalts. And this is all we can ſay in general of the manner in which ſalts are affected by heat.

In relating the effects of mixture upon ſalts, we can only mention at preſent the effects of mixing them with water, and ſome of thoſe produced by mixture with one another. Water is the only body which, in the way of mixture, produces effects upon ſalts that can be conſidered as general, or which are ſimilar in a certain degree on all the different ſalts. And one of theſe effects has been mentioned already in defining ſalts, when we ſaid they are bodies ſoluble in water.

All ſaline ſubſtances, when thrown into water, diſſolve in it more or leſs readily. This is ſo well known and familiar a property of all ſalts, that it is inſeparably connected with the idea of ſalt in the mind of a chemiſt.

There is therefore an attraction between water and ſalts, a diſpoſition to combine by chemical union. The only difference of ſalts from one another in this reſpect is in the force of this attraction, which is very different in the different ſalts. It is in ſome ſo powerful, that they attract the humidity which is contained in an imperceptible ſtate in common atmoſpherical air, and are diſſolved by that humidity into a liquid. Such ſalts, therefore, are called deliqueſcent ſalts, as I remarked formerly; and it requires the utmoſt care to keep them in the ſolid form. They muſt be kept in veſſels very accurately cloſed.

When we diſſolve a ſalt in water, we have an opportunity of obſerving ſeveral phenomena:

1. The firſt which occurs is the ſeparation of part of the air which the water contains, and that which generally adheres to the ſalt. The moment the ſalt diſſolves in the water, this fluid appears white or turbid; and when we examine the cauſe, we find numerous little bubbles of air, which in a ſhort time riſe to the top, and then the ſolution becomes tranſparent. This is an example of elective attraction or ſeparation of two bodies by means of a third. *(See Note 27. at the end of the Volume.)* This ſeparation of the air from the water is more ſudden and violent, if the water be made boiling hot before the ſalt is added; a violent momentary ebullition takes place, almoſt like an exploſion.

2. A ſecond particular obſervable when we diſſolve a ſalt in water, is, that the ſenſible heat of the mixture undergoes ſome change. In the greateſt number of caſes, the mixture becomes colder than the materials were before; but, in ſome caſes, it becomes warmer. Even the ſame ſalt, added in different ſtates to water, will increaſe the ſenſible heat of the water at one time, and diminiſh it at another.

The late Dr. Irvin ſuppoſed that the capacity of the mixture for heat is changed; and it appears to be ſo from ſome late experiments*.

3. A third particular which comes under our obſervation in diſſolving ſalt, is, that the more we diſſolve in the water, the more ſlowly it diſſolves the further additions of ſalt, until a certain quantity has been added; after which it diſſolves no more. The water is then ſaid to be ſaturated, and the ſolution is called a ſaturated ſolution. But this proportion of ſalt, which ſaturates water, is found to be very different, when we make the experiment, either with ſalts of different kinds, or with water of different degrees of heat. In trying the different ſalts, we find that each of them differs from almoſt every other. Some require twenty times their weight, or more, of the water to diſſolve them, while others are ſo ſoluble, as to diſſolve perfectly in leſs than an equal weight. And the greater number of thoſe very ſoluble ſalts ſhew, by other particulars, that they have a ſtronger attraction for water than other ſalts. Such ſalts attract it from moiſt air, and run

* The experiments on this ſubject were, in general, in this form: The heat required for changing the temperature of the materials one degree, was aſcertained; as alſo the heat neceſſary for changing that of the mixture. In ſeveral caſes, the laſt was found greater or leſs than the ſum of the two others, according as the mixture produced cold or heat. Hence Dr. Irvin was induced to confide more in the truth of his general principle. But Mr. Gadolin, and afterwards Lavoiſier, and De la Place, ſhewed that there was no ſteady connection between thoſe phenomena, the exceptions being equally numerous, if not more ſo; and the phenomena appear, as yet, altogether anomalous.

EDITOR.

per deliquium, becoming perfectly fluid, with a very small quantity of water. Moreover, these salts, without exception, retain the water more strongly than others, when we attempt to separate it by heat. The other salts, which are less soluble, or require much water, are not liable to deliquescence, and are more easily deprived of it by heat. But the solubility of most salts is greatly affected also by the heat of the water.

This is a curious subject, and the intimate knowledge of it requires, and leads to very nice speculations concerning the manner of chemical action. But I find that it will be long before we can safely proceed in this way. It is better for us to be contented with accurate and extensive observation of the facts. It will be no mean service to the chemist, to state, in a number of important cases, the proportion in which water dissolves salt at different temperatures. You will find a considerable collection of this kind by a Mr. Pfaèhler, in Rozier's Journal for 1775, and occasional observations by almost every chemical writer.

The most simple and unexceptionable form of the experiment is to throw into cold water a greater quantity of salt than it can dissolve. Having made a saturated solution in that temperature, pour it off into a vessel, in which it may be made warmer to any degree. Pour into this solution more salt than it can dissolve in that temperature, and pour off the saturated solution, to be treated in the same manner at a higher temperature, and so on. Having noted the quantity of these solutions, (by weight) and knowing the strength of brines of different weights, we obtain the degrees of solubility corresponding to different temperatures. The difference in some salts is very great. A salt, which you will afterwards know by the name of S. de Seignette, requires an ounce of water, of the temperature 50°, to dissolve 37 grains. But an ounce of boiling hot water dissolves 640 grains, nearly 18 times as much as the cold water. Nitre is about 7 times more so-

luble in boiling water, than at 50°. Glauber's ſalt is about 3½ times. Green vitriol is about 8 times, &c. &c.

My friend Dr. Hutton engaged in a ſeries of experiments of this kind, and conducted them in the form I have been deſcribing. He did not content himſelf with trials in two extreme temperatures, but took three or four for each ſalt. This ſhewed him that the progreſs of ſolubility was not equable in any ſalt, but that, in moſt of them, the ſolubility increaſed faſter than the temperature: For, when he took a temperature half way between the two extremes, he found that the difference between the quantities of ſalt diſſolved in the hotteſt water, and in that of mean temperature, was generally greater than the difference between this laſt and the coldeſt ſolution.

Hence it followed that the ſalt contained in a pint of the hotteſt, and a pint of the coldeſt, was more than was contained in two pints of the mean temperature, (all three being ſaturated ſolutions); and, therefore, if the two extreme ſolutions were mixed, the mixture ſhould not be able to keep the ſalt diſſolved. He found, accordingly, that it did not, but made an inſtantaneous, and, ſometimes, a copious precipitation. He alſo obſerved conſiderable deviations from the temperature which he expected on mixture. It were to be wiſhed that he had proſecuted theſe curious experiments; for this general law which he obſerved is a curious and important one. But his attention was called off by a moſt important conſequence which he deduced from it, namely, if the ſolubility of water in air obſerves ſuch a law, it follows, that two ſaturated ſtrata of air, of different temperatures, cannot mix without precipitation. On this he has founded his Theory of Rain; a theory which explains ſome of the greater changes in our atmoſphere more perſpicuouſly than any I know. *(See Note 28. at the end of the Volume.)*

This obſervation holds true with reſpect to moſt ſalts. Perhaps there is only one which we can ſay is an exception, viz. Common Salt. Macquer ſays that all deliqueſcent ſalts are exceptions; but

this is not juſt. Beſides, it is not eaſy to determine what ſalts are deliqueſcent, and what not.

From ſome of the particulars already ſtated, as obſervable in diſſolving the ſalts, you will perceive the foundation of an opinion of Sir Iſaac Newton concerning thoſe ſolutions. He ſuppoſed that the particles of a ſalt diſſolved in water are equidiſtant from one another through the whole of the water, and, therefore, arranged in a regular order, or, as he expreſſes it, in rank and file. This arrangement is, in reality, a neceſſary conſequence of ſome of the particulars we obſerved in the action of water upon ſalt. We find that water has a ſtronger attraction for ſalt, in proportion as it is purer, or contains leſs ſalt; for, in proportion to the quantity of ſalt it already contains, it diſſolves further additions of ſalt the more ſlowly. This is all the proof that we can deſire that it is acting upon them with a weaker attraction, or diſſolving power, until it have diſſolved a certain quantity, after which it cannot diſſolve any more. It is, therefore, evident, that if, in a ſolution of ſalt, any part of the water contain leſs ſalt than the reſt, that part muſt have a ſtronger attraction, and muſt draw ſome particles of ſalt from the others into itſelf, until the ſalt be diſtributed through the whole with the moſt exact equality and regularity; and the particles of it are, therefore, equidiſtant throughout the liquor.

When we examine the ſolidity of this reaſoning by an experiment, we have the pleaſure to find facts agree exactly with the theory; for, however unequally the ſalt be diſtributed in the liquor at firſt, it is ſure to ſpread from the parts which contain moſt, to thoſe which contain leſs, until it is equally diffuſed through the whole.

But it is proper, however, to obſerve on this occaſion, that when the liquor is preſerved from external diſturbance, this progreſs of the ſaline particles through it is exceſſively ſlow. This is the proof of the aſſumptions on the other ſide, namely, that an internal motion is neceſſary. It requires ſo long a time, that an impatient and inconſiderate

obſerver will perhaps conclude that it does not take place. But it is eaſy to explain why it requires ſo much time. There are two cauſes of this: *1ſt*, The weight of the ſaline particles, which are heavier than thoſe of the water, generally about twice as heavy; and, *2dly*, The very ſmall diſtance to which chemical attraction reaches round the particles of bodies. You will perceive the effect of theſe two cauſes, if you conſider with a little attention what muſt neceſſarily happen, in a ſolution in which the ſalt is unequally diſtributed through the water. Let us ſuppoſe, for example, that I put into a glaſs a certain quantity of a ſaturated ſolution of ſalt, and a quantity of pure water, without mixing them together by agitation, which will be beſt done by introducing the ſaturated ſolution under the water, by means of a funnel and pipe reaching to the bottom of the glaſs;—the ſolution being poured ſlowly into the funnel, will riſe quietly under the water, and they will be ſeparated by a ſurface perfectly even. The ſolution of a deep-coloured and heavy ſalt is the beſt for this purpoſe. Blue vitriol anſwers very well. It muſt immediately happen, that the loweſt part of the pure water, which is in contact with the upper ſurface of the ſolution, while it draws up ſome particles of ſalt out of that upper ſurface, muſt form a horizontal ſtratum, or layer of fluid, which will be leſs ſalt than the ſaturated ſolution, and which will be interpoſed between it and the pure water. No ſooner does this take place, than the loweſt part of this ſtratum will draw up more ſalt from the ſaturated ſolution upon which it lies; while at the ſame time, the pure water will draw up ſome ſalt out of the upper ſurface of the ſtratum. And thus innumerable ſtrata will be produced within a very ſmall ſpace, and in a very ſhort time,—all lying between the pure water and the ſaturated ſolution; the loweſt of which will contain leſs ſalt than the ſaturated ſolution, and each of the reſt leſs than that one immediately below it. But when matters are in this condition, every two contiguous ſtrata, which (on account of the extremely ſmall diſtance to which chemical attraction reaches) are the only ones that can act on one another, muſt act with an extremely

weak force. The upper one muſt draw ſalt from the one under it very weakly, for this reaſon, that they differ very little from one another in ſaltneſs, and therefore in attraction for ſalt. And, for the ſame reaſon, the pure water above will have but a weak power to draw the ſalt up from the uppermoſt and moſt diluted ſtratum. And now, if we conſider that the particles of the ſalt are heavier than thoſe of the water, it is eaſy to conceive how their gravitation, together with the reſiſtance they muſt meet with in moving through the fluid, muſt almoſt counterbalance the force with which they are drawn up, ſo as to render the motion exceſſively ſlow.

The ſlowneſs with which the equal diffuſion of a ſalt in water is performed, when left to itſelf, is therefore evidently a neceſſary conſequence of the very ſmall and imperceptible diſtance to which chemical attraction reaches, and alſo of the greater weight or gravitation of the ſalt than of the water *. And I thought it was worth the while to give a clear view of the matter, once for all, that you may underſtand better how to make ſolutions of ſalts, for different purpoſes, and the neceſſity of employing mechanical agitation to promote the equal diſtribution of the ſalts through the whole of the water, in a moderate time. In ſome of the arts, great miſtakes have been committed ſometimes, in conſequence of

* It is, indeed, the cleareſt proof of the nature of chemical action, and of the very ſmall diſtance to which it extends. It ſhews that it does extend to ſome diſtance, otherwiſe there would be nothing to move a particle out of its place. However ſtrongly particles attract one another in abſolute contact, this may cauſe them to cohere, but will never move a quieſcent particle out of its place. I muſt at the ſame time obſerve, that ſome ſolutions of ſaline ſubſtances manifeſt an inequality in their ſaturation, even after we have reaſon to think them perfectly uniform. When a ſtrong ſolution of ſal ammoniac, or of ſaccharum ſaturni, has been made perfectly uniform by agitation, it becomes unequal, in a ſmall degree, by long keeping, the lower ſtrata containing more ſalt than the upper. It is accompanied by a very curious circumſtance. If the ſolution be poured into a hollow glaſs priſm, having the angle downwards, and it be allowed to remain undiſturbed for ſome months, it acquires a double refraction, like rock cryſtal.

EDITOR.

the ignorance of this neceſſity. In the bleaching of linen, for example, and ſome other arts, the vitriolic acid is employed in part of the proceſs, very largely diluted with water. A careleſs workman may think it enough to pour in the proper quantity of acid, not recollecting that its great weight will keep it at the bottom, and that if it be not ſtirred briſkly about, it may be long ere it be equably diffuſed through the water. When goods are put in to be foured, they get into this very acid and corroſive part of the liquor, and are frequently eaten into holes by it.

Remember, therefore, when you diſſolve or dilute ſalts in water, to quicken the equal diſtribution of the ſalt by agitation; and be aſſured, that if there be enough of water, the ſalt will remain for ever after equally diffuſed throughout the whole.

It is ſomewhat ſurpriſing that the ſolutions of many ſalts begin to boil with a leſs heat than pure water does. But they ſoon riſe to a temperature conſiderably above that of boiling water, and nearly the ſame in them all. I obſerve that this firſt boiling is ſudden and tranſitory, ſo that I think it probable that it is the diſengagement of air which adhered to the ſalt. I obſerve, that on pouring boiling water on a ſolution of ſalt, or throwing ſalt into boiling water, there is a ſudden puff of ebullition like an exploſion.

Moſt of the ſalts may be ſeparated from water by evaporation, becauſe the greater number are much leſs volatile than water, and do not adhere with ſuch great force as greatly to diminiſh the volatility of the water.

But this evaporation may be conducted in two ways, either briſkly and uninterrupted, till all the water is diſſipated; or partially, and by repeated operations. The conſequence of the firſt method is, that the ſalt remains, forming a white maſs like chalk, or hardened lime-mortar, adhering more or leſs to the veſſel in which the evaporation was performed.

This is called EVAPORATION TO DRYNESS, and is attended, in moſt

caſes, with the loſs of ſome of the ſalt, which evaporates with the water, in conſequence of their mutual attraction.

But the ſecond method is that which is commonly followed, at leaſt with regard to a number of ſalts which are prepared in large quantities for ſale, ſuch as Glauber's ſalt, ſaltpetre, alum, vitriol, and others: And the conſequence of this method of proceeding is, that the ſalt is obtained in a different form. This method conſiſts in evaporating a ſolution to a certain degree, ſo that though the water which remains is ſtill ſufficient to retain all the ſalt diſſolved, as long as it is kept warm over a fire, it is too little, however, to retain it all diſſolved when it is cold. If it be taken off, therefore, and allowed to cool, a part of the ſalt muſt, and does ſeparate.

A ſalt depoſited in water in this manner is always formed into angular maſſes, having poliſhed ſurfaces, more or leſs of a regular figure, and tranſparent. Theſe maſſes are called CRYSTALS, from their reſemblance to the natural ſtones of that name. And one of the moſt curious particulars belonging to this ſubject is, that each diſtinct ſpecies of ſalt has a figure of its own, which it affects in its cryſtals, not always with exact regularity, but with ſuch a degree of it, that, upon the whole, a perſon well accuſtomed to view the cryſtals of different ſalts, will in moſt caſes be able to tell, by the figure and appearance of the ſaline cryſtals, from what ſalt they were produced. Nevertheleſs, a judgment formed in this way is far from being ſo ſure as has been imagined.

This method of ſeparating ſalts from water is called CRYSTALLIZATION, from its effect: And as it is practiſed often, you will find in books ſeveral directions for performing it right. But it is difficult to give any that will ſuit all the different kinds of ſalts, as they require different conduct. The common direction is to evaporate TO A PELLICLE; that is, till the ſurface of the ſolution exhibits a thin film, which gliſtens when viewed obliquely; and, when narrowly examined, is found to conſiſt of minute cryſtals. It is formed on the

ſurface, becauſe the gentle evaporation goes on there. But if you proceed ſo far with the evaporation, few of them will cryſtallize right, as too little water will remain, too much ſalt will be depoſited, and the cryſtals will be ſmall and confuſed. In general, it is uſual to filtrate the ſolution, if it be foul; becauſe a particle of ſolid matter any where in the fluid, becomes a centre of cryſtallization, (as may be obſerved in the threads hung in ſyrop for the manufacture of ſugar-candy) and this happening all over a ſolution that is foul, makes the cryſtals ſmall and confuſed. We muſt then evaporate it to a moderate degree, cool it ſlowly, and avoid diſturbance; and, pouring off the liquid, we repeat the ſame operation on it. Thoſe who prepare very large quantities of particular ſalts for ſale, as Glauber's ſalt, ſaltpetre, &c. have better, or at leaſt larger, cryſtals, on account of the large quantity, and ſlow refrigeration, than can eaſily be obtained from ſmall quantities in the way of experiment.

But ſome of the ſalts do not afford good cryſtals by this method, and agree better with an uninterrupted, but ſlow and inſenſible evaporation, with a heat like that of the human body, or with a ſpontaneous evaporation, merely by expoſure to the air. In this way the water is diminiſhed in ſo ſlow and gradual a manner, that the ſaline particles, in approaching one another, have full time and opportunity to aſſume that arrangement and mode of concretion, to which they are naturally diſpoſed. The cryſtals, therefore, are formed more regular and fine than by other proceſſes; but this method for obtaining cryſtals is far too ſlow for common uſe.

I am informed that ſome of the trading chemiſts in London, who prepare ſpecimens for the curious, have particular ſecrets by which they procure large and fine cryſtals, and that theſe ſecrets conſiſt in the addition of certain fatty matters, and making the ſolution in lime-water, and cloſe veſſels. It is certain, that in the great manufactures of ſaltpetre, alum, copperas, borax, &c. things of this kind are uſed with ſucceſs. Urine is a very general addition; and I am aſſured that

fine cryſtals of copperas and borax cannot be had without it. I have tried lime-water, and it certainly gives moſt elegant cryſtals of ſalt-petre, ſuperior to any that I can obtain without it. But it ſeems to have no ſuch effect on Glauber's ſalt, ſoda, or Epſom ſalt, the only other ſalts on which I have tried it.

Some authors ſpeak much of the influence of light on the formation of cryſtals, but I obſerve none *. Others ſay that a maſs of cryſtallized ſalt being placed in contact, or even very near the outſide of the veſſel, greatly affects the cryſtallization. I am perſuaded that this is a mere fancy. The directions I have already given are the only rules that have been generally ſucceſsful with me, and I think they are ſupported by any little knowledge that we have acquired of the internal procedure.

In evaporating ſolutions of ſalts, eſpecially when the evaporation is very ſlow, an accident often occurs which may give ſurpriſe, and may prove troubleſome to a perſon who is not accuſtomed to it;—I mean what is called the EFFLORESCENCE or VEGETATION of ſalts. This is a peculiar concretion of ſalts from water. When the evaporation is carried on in a deep veſſel, ſo as the concretion cannot riſe ſo high as to get over the lip, it ſhoots and ſpreads ſometimes in the upper part of the glaſs into branched figures, reſembling irregular foliage. And ſome of the more enthuſiaſtic chemiſts have imagined that they perceived in it an exact reſemblance to the leaves and ramifications of plants; and, if the ſalt was obtained from a plant, they ſuppoſed that it ſtill retained ſomething of the vegetating nature of the plant from which it was produced. But this is an imaginary, and perfectly groundleſs fancy. The reſemblance between ſuch concretions of ſalts and vege-

* Chaptal has, I think, proved it beyond contradiction, in a memoir preſented to the Royal Academy, 1780. I am certain of its influence in the cryſtallization of the vapours of ſalt of hartſhorn, and of camphor. Theſe ſalts are alſo ſingularly affected by electricity.

EDITOR.

tation is accidental, like that of the frozen humidity which is condenſed on the inſide of glaſs windows in hard froſt. This depends on the ſame cauſe as cryſtallization.

This vegetation, as it is called, is often troubleſome to the chemiſt. After reaching the lip of the veſſel, it creeps down the outſide, and, as ſoon as it get as low as the ſurface of the liquor, the whole becomes an aſſemblage of ſyphons, and our ſolution runs over and is loſt. This may be prevented by covering the veſſel with a piece of tin plate, having a round hole cut out of it, about two inches leſs in diameter than the rim of the evaporating diſh. This prevents the evaporation at the edges, and even occaſions a dew, which will trickle down the ſide of the diſh. I find this a complete preventive.

It happens ſometimes, in attempting to cryſtallize ſome ſalts, that although the liquor be duly evaporated, and then allowed to cool, it does not cryſtallize; the whole remains ſtill fluid. This happens only with ſome of the ſalts, and only when the ſaline liquor is allowed to cool ſlowly and without being diſturbed. If we diſturb it when it is thus cooled, it ſuddenly cryſtallizes, and at the ſame time becomes warmer by ſeveral degrees. It is therefore evident that its protracted fluidity proceeded from a quantity of heat, which it retained in the form of latent heat, in conſequence, I am perſuaded, of a chemical attraction which the materials of the mixture have for heat in that ſtate. As this is a curious and inſtructive experiment, you will be the better of a few inſtructions for making it ſucceed in the beſt manner. Glauber's and Epſom ſalts are thoſe which exhibit it to moſt advantage. Alum and copperas alſo do very well. Take cryſtals that have been formed in a ſolution carefully filtrated, that there may be no foulneſs; put them into a flaſk with diſtilled water, a little more than enough for diſſolving the whole with a boiling heat; ſet the flaſk into a pan or tea-kettle of cold water, and ſet the whole on the fire, and keep it boiling till all the ſalt is diſſolved; take out the flaſk from time to time to agitate the contents, otherwiſe it will conſiſt of ſtrata too une-

qually ſaturated: Now cork the flaſk, and let it ſtand in the boiling water for ſome time. I think that this allows a ſmall degree of more ſaltneſs towards the bottom, which I think of ſervice. Now ſet the whole in a cool place, where it will not be expoſed to the tremor of perſons walking in the room, or of carriages in the ſtreet. It will cool very ſlowly, and generally remains fluid. Lift the flaſk, with the utmoſt care not to ſhake it; pull out the cork, and it ſhoots into cryſtals; or, if it do not, drop into the liquor the ſmalleſt fragment of the ſalt in cryſtals, and thus it never fails. A thermometer will ſhew the emiſſion of latent heat. There is in this experiment an equilibrium in the mixture, between its chemical attraction for latent heat, or the force with which it retains a certain quantity of heat in that form, and the coheſive attraction which tends to make it cryſtallize. Theſe two attractions, the chemical and the coheſive, are always in oppoſition to one another, and here they are exactly balanced, or at leaſt the force of the chemical attraction for the heat, exceeds, by very little, the force of the coheſive, which tends to cryſtallization. A proof of this is, that if we give a little advantage to the coheſive attraction, either by ſudden concuſſion of the fluid matter, or by dropping in ever ſo little of the ſame matter already concreted, the cryſtallization immediately begins, and the latent heat is expelled; the coheſive attraction having now prevailed over the chemical one. You will perceive, therefore, an exact ſimilarity between this experiment and Fahrenheit's experiment with overcooled water *.

* I doubt much whether this reaſoning be concluſive or explicatory. We can form no notion of a difference between a coheſive and a chemical attraction, viewing them merely as moving forces; a conception abſolutely neceſſary for ſpeaking of oppoſition and equilibrium. It will be ſeen preſently, that what is here called a coheſive attraction, is an attraction as truly chemical as the attraction for heat; and indeed, in the opinion of many chemiſts, it is the only chemical attraction, uniting two bodies of different kinds, with mutual ſaturation, and the loſs of the former properties of the ingredients. We ſhall at leaſt ſee that more happens than the mere balancing of two attractions.

EDITOR.

Salts are commonly exhibited and treated of by chemiſts in their cryſtallized ſtate. But this is not the moſt ſimple ſtate of ſaline matter. All of them contain water, ſeparable by heat; and it is ſo copious in ſome, that they undergo WATERY FUSION, and ſpontaneous CALCINATION, improperly called Efflorescence. Glauber's ſalt, alum, copperas, and many other ſalts, when very ſuddenly heated, melt, boil and foam, emitting much watery vapour, and, in a little, are changed into a dry ſpongy maſs, much bigger than the ſalt, but of much ſmaller weight. After this they bear to be heated red hot, before they take the true wax-like fuſion which I deſcribed in the beginning. This is called the watery fuſion of the ſaline cryſtal. It is really a ſolution again in water which had been enabled, by its combination with the ſaline matter, to bear a greater heat without evaporation. It therefore diſſolves what it could not diſſolve while of a lower temperature: But it is almoſt inſtantly raiſed to a temperature in which even this union cannot hinder it from boiling. It therefore froths up, evaporates by burſting the ſaline bubbles, and leaves them ſhattered, and the w holea ſpongy maſs.

But if the ſame ſalt be expoſed to a very gentle heat, and the action of dry air, the water of cryſtallization evaporates from it without diſſolving the ſalt, and leaves it a fine meal. It is this that is called EFFLORESCENCE. The particles of this duſt appear, through a microſcope, to be fragments of inconceivably thin plates. In all probability, a ſaline cryſtal is made up of ultimate plates of ſalt and water *. In other ſalts, the water occaſions DECREPITATION, that is, the cryſtals retain the water with great obſtinacy, till red hot, or near it, when it tears them aſunder with a loud crack. Such, therefore, is the nature of the cryſtals of ſalts, that they muſt be conſidered as compounds of ſalt and water.

* This ſtructure is rendered more probable, by remarking that ſaline cryſtals are evidently ſtriated tranſverſely, and many of them have a double refraction, like ſome natural cryſtals, which we know to have a plated ſtructure. EDITOR.

The manner in which ſaline cryſtals are produced, and the remarkable figures they aſſume, could not fail to attract the attention of chemiſts and others, and to occaſion attempts to explain how they are formed.

The moſt obvious idea, and which firſt occurred, was, that the ultimate atoms of ſalts have ſimilar forms to thoſe of their cryſtals, being oblong, or angular, or pointed; and that, when they unite, they neceſſarily form a maſs, the figure of which is ſomewhat ſimilar to that of the conſtituent particles. This opinion appeared the more probable, as it was thought to explain the *diſſolving* power of ſalts, with regard to earths, metals, and other bodies, which, it was imagined, they diſſolved, or acted upon, in conſequence of the pointed forms and ſharpneſs of their particles. But as, when we conſidered the more general effects of mixture, we already found this an inſufficient and unſatisfactory explanation of the diſſolving power of ſolvents in general, ſo neither will it explain the cryſtallization of ſalts. It is certain, that, in the cryſtals of ſalts and ſaline compounds, the ſaline atoms do not touch one another. This is evident, in the firſt place, from the tranſparency of thoſe cryſtals. Light paſſes through them in every direction. We have direct proof that the ſaline atoms do not touch one another. The cryſtals of foſſil alkali, Glauber's ſalt, Epſom ſalt, vitriol, alum, and borax, contain a quantity of water more than thrice the bulk of the ſaline matter. Perfect cryſtals of foſſil alkali contain $\frac{64}{100}$ of water, $\frac{16}{100}$ of fixed air, and $\frac{20}{100}$ of alkali. The form of the cryſtals, therefore, cannot depend merely on the form of the ſaline atoms.

Beſides, this phenomenon of cryſtallization is not confined to ſalts. They are of all other ſubſtances the moſt eaſily diſpoſed to cryſtallization; but moſt other kinds of matter, when paſſing from a ſtate of fluidity to ſolidity, ſhew more or leſs diſpoſition to concrete into regular figures. In mineral veins, we find many kinds of ſtones and minerals cryſtallized. Metals, in congealing, ſhew a diſpoſition to cryſtallize,

or to form regular figures. Pure water alſo cryſtallizes in ſnow and ice. Flakes of ſnow are formed of aſſemblages of ſmall ſpicular or columnar cryſtals, like the cryſtals of Glauber's ſalt, often aſſembled together without order, but ſometimes joined together into ſtars of ſix rays, &c. If we ſuppoſe, therefore, the figures of ſaline cryſtals to be a proof that the ultimate atoms of ſalts are angular and pointed, we muſt allow the particles of water to be ſo likewiſe.

Another principle was pointed out by Sir Iſaac Newton, to explain the cryſtallization of ſalts, but it is inſufficient. He ſuppoſed it to be a conſequence of the regular arrangement of the ſaline atoms diſſolved in water. This might explain the concretion of ſalts into a maſs of uniform ſtructure, and perhaps tranſparent; but it cannot explain why there are a number of ſeparate angular maſſes.

In the firſt place, the cauſe which occaſions the particles of ſalt to concrete together in cryſtallization, is undoubtedly the attraction of coheſion. In a ſolution of ſalt, this attraction, by which the particles of the ſalt have a tendency to unite, is counteracted and overcome by the chemical attraction of the water for the particles of the ſalt. But if we diminiſh the quantity of the water by evaporation, it will act with leſs power, and the ſaline particles with more. *(See Note 29. at the end of the Volume.)*

Mr. Baumé's opinion is, that cryſtallization, that is, the ſeparation of ſalts by cryſtallization, depends both on the attraction of the homogeneous particles for one another, and on the repulſion of the heterogeneous ones; and he thus explains how *perfectly neutral cryſtals* are formed in acid, or alkaline ſolutions; or clean and tranſparent cryſtals in muddy and coloured ſaline liquors. And he thinks he has perceived, by experiments, both the attraction and repulſion, acting even at the diſtance of a foot, by placing beſide a veſſel, in which a ſolution of Glauber's ſalt was ſet to cryſtallize, either a veſſel containing Glauber's ſalt, which occaſioned the cryſtals to be all formed on that ſide; or a veſſel containing ſalt of tartar, which occaſioned the cryſtals to be

formed on the oppoſite ſide. But Mr. Lavoiſier has proved that there is not the leaſt ground for thinking that the attraction and repulſion act at a ſenſible diſtance.

Conſult, on this ſubject, the *Cryſtallographie* of Mr. Romè de l'Iſle, edition 1784; and the *Eſſai ſur la formation des Cryſtaux, par* Mr. l'Abbé de Haüy. (*Annales de Chemie, Juin* 1793.) Theſe authors, particularly the laſt, ſhew how, from a very ſmall number of ſimple primitive forms, may ariſe a vaſt variety of figures of cryſtals. This ſubject is treated with great neatneſs and perſpicuity, both mathematically and philoſophically, in a work intitled *Cryſtallographie des Mineraux*, by Dr. Kramp of Straſburg. It is printed at Vienna 1793, and is, I think, the moſt inſtructive work on the ſubject.

There is yet another way of ſeparating ſalts from water, namely, by adding ſomething with which the water is more diſpoſed to unite. Thus, if to a nearly ſaturated ſolution of moſt ſalts in water, we pour ſome ſtrong ſpirit of wine, we ſhall have an immediate precipitation of the ſalt in a cryſtalline form. This method is ſometimes practiſed as the only way of procuring the ſalt in a ſtate of great purity. It is ſurely ſomewhat extraordinary, that by the addition of a ſubſtance more ſtrongly attracted by the water, we ſhould produce, in an inſtant, the union of ſalt and water in a cryſtalline form, ſo ſtrong in ſome caſes, that a red heat cannot ſeparate them; and this is another proof how little we are able to judge of the comparative ſtrength of chemical attractions.

Such are the general obſervations which it was proper to make on the relation of ſalts to water.

All the different kinds of ſalt may be eaſily mixed in the moſt intimate manner, in conſequence of this relation,—this general quality of diſſolving in water: For water can diſſolve not only two kinds of ſalt at the ſame time, but many different kinds. And we have learned by experience, that when it is ſaturated with one ſalt, this does not hinder from diſſolving a conſiderable quantity of another, and even of

ſeveral others. You may ſee many examples of this collected by Muſchenbroeck, Neuman, Watſon, and others. Water will diſſolve more ſaltpetre after it is ſaturated with alum than when pure.

When different ſalts are thus mixed together by being diſſolved in the ſame water, ſome act immediately one on another, and unite together, ſo as to remain afterwards very ſtrongly combined. Many others mix without any appearance of action, or any ſigns of their uniting together. Theſe can generally be ſeparated from one another again without much difficulty. The others, when we deſire to ſeparate them, require to be diſunited by an elective attraction. The various caſes, where elective attraction or exchange is neceſſary to ſeparate ſalts which are ſtrongly united together, will be fully conſidered hereafter.

But when the mixed ſalts have no attraction, or after that attraction has been overcome, we ſeparate them by one or other of three methods, which will ſucceed in moſt of theſe caſes. We either take advantage of their difference of volatility, if there be any conſiderable difference of this kind between them; or of their different ſolubility in water; or we ſeparate them by cryſtallizing them.

The manner of ſeparating them by taking advantage of their different volatility, when there is a conſiderable difference between them in this reſpect, is obvious. It is to put the ſaline mixture into a retort, or other ſuch veſſel, to join a receiver to this retort, and then to apply heat, which muſt be very ſlowly and gradually increaſed. Thus the more volatile ſalt may be raiſed to vapour, and condenſed in a ſeparate veſſel, while the fixed ſalt remains behind. In no caſe does this proceſs produce a complete ſeparation. The volatile ſalt is always tainted with a little of the fixed one, and this always retains ſome of the volatile ſalt. We muſt often repeat the operation two or three times.

When the ſalts which we deſire to ſeparate do not differ much in volatility, and this operation cannot be performed, it happens however in many caſes that they differ greatly in ſolubility; and this

circumſtance puts it in our power to ſeparate them. Thus, if the one be one of the leſs ſoluble ſalts, and require a large quantity of water to diſſolve it, while the other eaſily diſſolves in a ſmall quantity of water, we may evaporate the mixed ſolution till no more water remain than is neceſſary to keep the moſt ſoluble ſalt diſſolved. Allowing the ſolution then to cool, the leſs ſoluble ſalt will ſeparate in cryſtals, and may be taken out, while the other remains ſtill diſſolved.

Or, if the ſalts be mixed together in a dry maſs, we may add as much water as is juſt ſufficient to diſſolve the moſt ſoluble, and the leſs ſoluble will remain undiſſolved. When one of thoſe ſalts has little ſolubility, and the other is ſo very ſoluble as to be deliqueſcent, we can obtain a more complete and exact ſeparation by expoſing the mixed maſs to the air in a cellar, or ſome other damp and cold place, until the deliqueſcent ſalt be liquefied by attracting the moiſture of the air. It is found by experience that there is no method of diſſolving it by which it is ſo effectually parted from the other. Sometimes the mixed maſs is thus expoſed upon a quantity of bibulous paper, or other porous abſorbent ſubſtances, which ſuck in the deliqueſcent ſalt as faſt as it liquefies: And this is a moſt effectual and quick method.

Beſides the difference of greater or leſs ſolubility in general, there is another difference of ſolubility, which is in ſome caſes the foundation of *ſeparability*, (if I may be allowed the term); I mean a difference of ſolubility in hot or in cold water. Thus, common ſea-ſalt differs from perhaps all the reſt, by diſſolving as readily, and almoſt as copiouſly, in cold water as in hot, and may therefore be very conveniently and effectually ſeparated from many of them, upon this ſame principle. The ſaltpetre, in its native ſtate, has always a great quantity of common ſalt mixed with it. In order to ſeparate it, the whole is diſſolved in water, and the ſolution evaporated to a certain degree, and then cooled. A quantity of ſaltpetre then cryſtallizes, and is taken out. The ſame operation is repeated with the remaining

liquor, and ſome more ſaltpetre is thus taken out before any of the common ſalt cryſtallizes, becauſe the common ſalt is generally the leaſt copious of the two; and as it is alſo more ſoluble, the water muſt be conſiderably waſted before it begins to appear. But when the evaporation has been repeated a certain number of times, the remaining water comes to be ſaturated with common ſalt as well as with ſaltpetre. And now, the next time we evaporate, a part of the common ſalt will cryſtallize, but in a manner quite different from the ſaltpetre, for it cryſtallizes while the evaporation is going on, and while the liquor is hot on the fire; for the heat does not much increaſe its ſolubility, or prevent it from ſeparating. But heat has this effect very remarkably upon the ſaltpetre. It is diſſolved in greater quantity by far in hot water than in cold, and although the water be greatly diminiſhed, it ſtill remains diſſolved as long as it is hot. The common ſalt which has grained, muſt now be taken out; and afterwards we muſt allow the ſolution to cool. While it cools, none of the remaining common ſalt ſeparates, or exceſſively little; but a great part of the ſaltpetre now concretes in the form of cryſtals: And if theſe are taken out, the ſame operation may be repeated with the remaining ſolution, until the whole of theſe ſalts are alternately ſeparated from the water, and from one another; the one always concreting while the fluid evaporates, and the other while it is cooled. And the very ſame operation may be practiſed to ſeparate ſea-ſalt from a number of the other ſalts.

Solubility in ſpirit of wine is alſo another foundation of ſeparability; for ſome ſalts are diſſolved by that fluid, while the reſt are not: but of this we ſhall ſpeak more fully when we treat of ſpirit of wine.

In caſes in which none of the methods hitherto mentioned will do, (which however are very few), the only remaining means is cryſtallization. Some of the former methods of managing might be claſſed under the title of cryſtallization, becauſe the ſeparated ſalts form cryſtals; but as the ſeparation depends in reality upon difference of ſolubility, we have referred ſuch caſes to that head.

The caſes in which the ſeparation of ſalts depends on cryſtallization alone, are thoſe in which the mixed ſalts do not differ conſiderably, either in volatility or ſolubility, and in which we cannot have recourſe to any of the operations hitherto mentioned. All that we have left in our power is to reduce the whole ſaline matter to cryſtals, in the beſt manner we can; that is, to give fair cryſtals, but this is the moſt imperfect mode of any. *(See Note 30. at the end of the Volume.)*

To conclude this ſubject of the ſeparation of ſalts in general, we muſt remark, that ſome of theſe methods of working do not produce full and exact ſeparation at firſt; but by repetition the ſeparation can be made very complete.

You are now acquainted with the nature of the ſalts in general: We muſt next take a view of the different kinds of them. To do this in a proper manner, it is neceſſary firſt to divide them into two principal diviſions: the firſt, that of the more ſimple; the ſecond, that of the compound ſalts, which are formed by the union of the ſimpler ſalts with one another. We ſhall therefore conſider the ſimpler ſalts firſt, and then ſhew how by combination they produce the compound: Each of which ſhall be conſidered ſeparately, as well as the means of reſolving the compound again into its conſtituent ſalts.

Further, the ſimpler ſalts, that they may be conſidered more diſtinctly, muſt be divided into two orders, the alkaline, and the acid ſalts. You will be fully ſenſible of the propriety of this diviſion as we proceed. At preſent we are to conſider the alkalis.

GENUS I.

ALKALINE SALTS.

UNDER this general name are comprehended three ſalts, having ſimilar properties, but, at the ſame time, other properties which are not interchangeable.

The only character which I need to give of them in common, at preſent, is, that when they are mixed, though in exceedingly ſmall quantity, with infuſions of purple or blue flowers of vegetables, they change the colour generally to a green, or, in ſome few caſes, from purple to blue. The ſpiritous tincture of alkanet root is ſtill more ſenſible to alkalis. Several other properties beſides this are uſually given to diſtinguiſh the alkalis, but it is not neceſſary to notice them at preſent; it is better to delay the mention of them now. I may, however, add, that, when taſted as diſſolved in water, they affect the tongue with conſiderable acrimony and pungency. The particular taſte is much the ſame in the three ſpecies *. They are, moreover,

* This taſte has not got a name in our language, nor is it indeed very familiar, being peculiarly diſguſting, and is ſeldom to be met with, except in what we are accuſtomed to conſider as offal, or refuſe. But it is a taſte as diſtinct and characteriſtic as the acidity which denominates the other claſs. Whoever touches, with the tip of the tongue, a little potaſh, or even lime, or lime water, will never afterwards forget the diſguſting taſte of an alkaline ſubſtance.

EDITOR.

noted detergents, and feel greaſy to the touch, becauſe they inſtantly diſſolve the unctuous matters which adhere to the ſkin. They ſhew, indeed, an acrid and corroſive nature with reſpect to all animal ſubſtances, eſpecially when we apply to them theſe alkalis in their pureſt and moſt active ſtate, in which condition they diſſolve every thing into a ſoapy pulp. It is a ſtate to which they are reduced on ſome occaſions only, and to ſerve particular purpoſes. But it is better, for the preſent, to deſcribe them in the ſtate in which they are commonly kept for uſe, and in which you will moſt frequently ſee them in the ſhops. This is a more convenient and manageable form than their pureſt ſtate.

I ſaid that there are three ſpecies of them. Theſe are the vegetable alkali, the foſſil alkali, and the volatile alkali. The laſt is called volatile very properly, as it is incomparably more volatile than the other two, which, with reference to it, are therefore called the two fixed alkalis.

SPECIES. I.—VEGETABLE ALKALI.

THE firſt, the vegetable alkali, or common potaſh, has all the qualities deſcribed as belonging to ſalts in general. It requires a red heat to melt it, and in a violent one emits vapours, and is gradually diſſipated, eſpecially if air be admitted to its ſurface.

This alkali, in its ordinary ſtate, has a very ſtrong attraction for water. It deliquiates in the air, and is with great difficulty kept dry. When thrown into water, it diſſolves quickly, and can be diſſolved in leſs than an equal quantity. The ſolution is generally accompanied with heat. The water is ſeparated again with difficulty, and requires a ſtrong heat; and it is not eaſy to obtain cryſtals from this ſalt in its ordinary ſtate. It is not uſual, therefore, to attempt to cryſtallize this alkali, in ſeparating it from water. The common practice is to eva-

porate to dryneſs, which is done in an iron pot, as it has ſcarcely any power to corrode the iron. Hence it has got the name of potaſh.

When the greater part of the water is boiled away, the remaining matter is much diſpoſed to foam and boil over; and at laſt the ſmall portion of water which remains with the ſalt, forms with it a mixture like wet lime or ſoft mortar. A ſtrong heat is then required to evaporate this laſt part of the water; and the ſalt muſt be ſtirred and ſcraped from the bottom of the pot inceſſantly. If this be omitted, it adheres ſtrongly in the form of a hard white cruſt, or indurated maſs.

This alkali has its origin in the land vegetables, eſpecially trees, the greater number of which contain, in their juices, a ſmall quantity of this ſalt combined with others; and it is produced in the plant by the powers of vegetation.

It is extracted, for the moſt part, by burning the wood (or whatever plant we operate on) ſlowly, to perfect white aſhes, or until all the inflammable matter be completely conſumed. The alkali remains in theſe aſhes and is eaſily extracted from them by water; and when refined to a certain degree, is ſold under the name of Pearl Ashes.

It is prepared in this manner chiefly in thoſe countries where wood is exceedingly plenty and cheap. And for many purpoſes, they do not take the trouble to refine this ſalt, but, taking the ſolution of it which they have extracted by water from the aſhes, they add to it a quantity of freſh aſhes, which ſtill contains a ſalt, and boil up the whole together into a coarſe and impure maſs, which is alſo called Potashes, and from which the alkali is afterwards extracted by thoſe who have occaſion to uſe it.

The reaſon for ſelling ſome of it in this form is, that it is more eaſily preſerved and tranſported. The purer alkali, called pearl aſhes, is ſo deliqueſcent, that it would be impoſſible to keep it dry and ſolid, except by packing it up in very cloſe and tight veſſels; whereas the coarſer or leſs pure kinds of potaſhes can be more eaſily preſerved and tranſported without ſuch expenſive package.

The ſofter and more ſucculent vegetables do not contain ſo much of it as the hard woods, but ſome of them, eſpecially ferns and ſome others, are burnt with great profit in ſome places, on account of the alkali which may be got from their aſhes; and ſuch aſhes are ſold under the name of weed-aſhes.

It is alſo ſeparated, in a great meaſure, from the other principles of vegetables, when they are completely putrefied, and in this way is produced the alkali, which may be got from the drains of dunghills, by a proceſs deſcribed by Dr. Percival of Mancheſter, in the Philoſophical Tranſactions for the year 1779 or 1780.

There is now ſuch demand for this ſalt in many manufactures as a detergent, that it is found highly worth while to recover it from the liquors in which it is mixed with all the ſordes which it has diſſolved. The water is boiled off, and the impure reſiduum is burnt to aſhes, and the alkaline ſalt extracted from them by water.

Mr. Margraaf, in ſome memoirs in the Tranſactions of the Berlin Academy on the waters uſed in Berlin, ſays that he found a ſmall quantity of this alkaliin ſome of their well waters. It probably had the ſame origin as the alkali contained in the drainings of dunghills, and had been extricated from vegetable ſubſtances by putrefaction, and been carried by the ſurface water into the wells.

The vegetable ſubſtance which yields it in the largeſt quantity and pureſt ſtate, is the vegetable ſalt called tartar or wine-ſtone; and when this alkali has been extracted from the tartar, it has been commonly called *Salt of Tartar* *.

Sal tartari contains about 70 per cent of pure, i. e. cauſtic alkali.

The beſt pearl-aſhes contain about -	55
Dantzig aſhes - - -	51
Ordinary - - -	46

* There are ſtrong reaſons, ſome of which occur in theſe lectures, for believing that this ſalt is formed in the plants, by the powers of vegetation, of ingredients ſtill more ſimple. EDITOR.

The names, therefore, by which this alkali was formerly known, were pot aſh, pearl aſh, weed aſh, ſalt of tartar, ſalt of wormwood, and ſome others. But the names of the ſalts have been often changed of late. In a new ſet of names which were lately propoſed by the French chemiſts, it is called *Potaſſe* in French, and *Potaſſa* for Latin. In the laſt edition of the London Pharmacopœia, *Kali;* in the Edinburgh Pharmacopœia, *Lixiva.* I ſhall take ſome notice of the names when I have done with the deſcription of the ſalts.

Uſes of the vegetable fixed alkali.——It conſtitutes a part of ſeveral compound ſalts employed in medicine, and is uſed by itſelf to correct acidity in the bowels, to promote the ſecretion of urine, and to give relief from the diſtreſs occaſioned by gravel or the ſtone, in the kidneys or bladder; which relief it very ſeldom fails to procure, when the diſeaſe is not of long ſtanding. It is given for this purpoſe in different ſtates, which I ſhall deſcribe hereafter. It is uſed alſo in large quantities in the arts, as in making ſoap and glaſs, in bleaching, in dying, &c.

SPECIES II.—FOSSIL ALKALI.

The other fixed alkali, the foſſil alkali, when examined in its ordinary ſtate, reſembles the vegetable alkali in ſome particulars; but it is very different from it in others. It requires the ſame heat to melt it and make it evaporate. It has the ſame taſte, but not ſo acrid. Its attraction for water is not near ſo ſtrong, and it is not deliqueſcent. It diſſolves, however, readily in water, but not ſo quickly as the vegetable alkali does; and it requires five or ſix times its weight of the water to diſſolve it, in the temperature 50°.

The foſſil fixed alkali in ſolution is eaſily ſeparated from water again by evaporation, and in its ordinary ſtate, it is eaſily cryſtallized. Its cryſtals have generally the form of a thin oblique lozenge, the narrow ſides of which are reduced to an edge by oblique planes, like the edge of a ruler. (See Rome de l'Iſle, No. 43.) Its cryſtals con-

tain a large proportion of water, adhering loosely, nearly $\frac{64}{100}$, and $\frac{16}{100}$ of elastic matter in an unelastic state. Hence they are very liable to watery fusion and spontaneous calcination. The water quickly evaporates, and leaves the salt a white crusty mass, which may be dissolved in water, and crystallized again.

Such are the qualities of this alkali, that can be mentioned with propriety at present. It remains only to take notice of its origin. Upon this article we may remark, that it is one of the salts which have been longest known and in use. It is this salt which is mentioned in several parts of the Bible by the name of nitre. It is once mentioned as a detergent, and once as making a turbulent or foaming mixture with vinegar, neither of which properties agree with the salt now called nitre. It appears that the ancients were supplied with this salt from Egypt, and some parts of Persia, where it is found on the surface of the soil, in particular places, in the form of small impure crystals, or composing a saline efflorescence, or crust, like hoar-frost, which is undoubtedly formed by the evaporation of water, which had dissolved and extracted it from the soil.

In the 55th volume of the Philosophical Transactions, there is mention made by Dr. Heberden, of a production of this alkali, somewhat similar to this, in the island of Teneriffe. And we are also informed by Dr. Donald Monro, in the Philosophical Transactions for the year 1771, of another source, from which it is probable the ancients derived in part their supplies of this salt. It is brought yearly to Tripoli, in large quantities from the mountains, and is called *Trona*. From its appearance, he concludes that it is found forming veins in the mountains of rock-salt; for it is always in the form of cakes, of about half an inch thick, to one side of which there always adhere incrusted crystals of common salt *.

* It is found in its detached state in all salt water fishes, and in some in considerable quantity. The earth near Ravenna, which is full of the remains of sea fishes, and even the stones, abound in it. EDITOR.

There is no example, however, in any part of Europe, of its being found in conſiderable quantity in any of theſe ſtates. We have only a ſaline effloreſcence, which is often formed on the plaſtered walls of cellars, and other damp places, which is moſtly compoſed of this ſalt. We alſo find a ſmall quantity of it diſſolved in the water of ſome of our ſprings in Europe; but ſuch ſpring waters are very uncommon.

Though, on the whole, we but rarely find this alkali in a ſeparate ſtate in nature, it occurs however, in great quantities, in a ſtate of combination with other ſalts. Common ſalt is a compound ſalt, of which this fixed alkali compoſes the half. And common ſalt is found ſolid, in immenſe quantities, in the bowels of the earth, beſides, what is contained in the waters of the ſea, and thoſe of innumerable ſprings in all parts of the world. You will, therefore, underſtand why it has been called the foſſil alkali. I have reaſon to believe that the foſſil alkali that is found on the plaſtered walls of cellars, and other damp places, is produced by the decompoſition of common ſalt.

But the great quantities of it which are made uſe of in different arts and manufactures in Europe, are got from ſea plants, and ſome particular plants which grow in the neighbourhood of the ſea. The plant which affords it of the beſt quality, and is cultivated in Spain, Sicily, and Italy, for that purpoſe, is called Kali and Salſola. To obtain the alkali, theſe plants are burned to aſhes, and the alkali is found in their aſhes. Such aſhes are accordingly prepared upon the coaſts of the Mediterranean, and receive the name of Soda, or Soude, or Barilla Aſhes. They form large and hard lumps like ſtones, of a dark grey or blackiſh colour. The hardneſs is occaſioned by a fuſion which theſe aſhes undergo in the end of the operation, in conſequence of the heat produced by the burning of the vegetable. Upon our coaſts alſo, we burn certain kinds of ſea plants, and melt the ſaline aſhes of them together into one maſs, called Kelp, which alſo contains a ſmall quantity of this alkali.

The pureſt foſſil alkali obtained from the efflores̄cence on plaſter walls, contains about 60 per cent. of its weight of alkali in cryſtals.

Alkali manufactured at Liverpool,	49
Foſſil alkali from India,	28
Beſt Alicant Barilla,	$26\frac{1}{2}$
Sicilian Barilla,	23
The richeſt kelp made in Norway, the Orkney Iſlands, and Skye,	$6\frac{1}{2}$
The general produce of Scottiſh kelp,	$2\frac{1}{2}$

The ſea-weed, varech, or wreck, is either taken as it is toſſed aſhore by the ſtorms, or is cut and pulled from the rocks with rakes, at low water. It is brought aſhore and ſpread on the beach to dry and wither. As ſoon as it will burn, it is burnt in little pits lined with ſtone, feeding the fire ſcantily with the dry plant. When the pit is nearly full of the aſhes, which are but ſlightly burnt, a great quantity of weed, well dried on purpoſe, is thrown in at once, and a very hot fire kept up, and the whole ſtirred, to bring all the charry matter to the air, that it may burn. This melts the whole into an impure heterogeneous maſs, of a dark dirty gray colour, containing much common ſalt, vegetable earth, brimſtone, and the alkali.

It may alſo be had by decompounding common ſalt, by particular proceſſes to be deſcribed hereafter, and ſome of which are actually practiſed in the way of buſineſs at preſent.

The names which have been given to it, therefore, are, Natrum or Nitrum by the ancients, Trona in Africa, Soda and Barilla in the Mediterranean, and Alkali of Common Salt, or Foſſil Fixed Alkali. In France they prefer the name of Soda or Soude, and the ſame name has been adopted in the laſt edition of the Edinburgh Pharmacopœia. In the London Pharmacopœia, it is named Natron.

It is employed chiefly in the compoſition of ſoap, in making glaſs, in bleaching linen, and in other arts. It is alſo an article of the ma-

teria medica. It has the ſame powers in medicine as potaſh, or alkali of tartar.

SPECIES III.—VOLATILE ALKALI.

The laſt of theſe ſalts, the volatile alkali, differs more from the two former than they do from one another. It ſhews great volatility when expoſed to the moſt gentle heat; and, even in the ordinary heat of the air, it is conſtantly emitting vapour, not viſible, but pungent to the noſe, and of a urinous ſmell. It is commonly known by the name of volatile ſalts, and ſalt of hartſhorn. When expoſed to heat, it does not melt, but is totally evaporated before it arrives at the melting heat; and when the vapour of it is condenſed in a receiver, it forms a ſaline cruſt. It has a good deal of acrimony and pungency, and, if confined upon the ſkin, will certainly inflame and corrode it. It is eaſily combined with water, and ſometimes cryſtallizes; but it is incapable of being ſeparated by evaporation, as other ſalts, by employing heat.

With reſpect to its origin, it is not found in a ſeparate ſtate in nature, except in putrefying animal or vegetable ſubſtances, or in the air which is immediately over them. It is contained in all animal and vegetable ſubſtances, and is extricated from them by putrefaction, and by a deſtructive heat. There is alſo a little of it in foſſil coals. Hence it appears in the vapours which ariſe from theſe different ſubſtances, when they are deſtroyed or conſumed by fire, or by a heat ſtrong enough to diſſipate all their volatile parts. In Lord Dundonald's proceſs for obtaining a tar from pit-coal, a quantity of water is got along with the tar, which water contains ſome volatile alkali. Soot, which is a matter produced by the condenſation of ſome of the vapours ariſing from heated coal or wood, alſo contains it, combined with ſome other ſaline ſubſtances, which repreſs its volatility to a cer-

tain degree. It is therefore extracted in large quantity in ſome manufactures from ſoot. Urine alſo, when putrefied to a certain degree, yields a good deal of it, and this is the cauſe of the pungent alkaline odour felt in ſtables. Some manufacturers therefore prepare it from urine, and the chemiſts formerly did ſo too, and called it ſalt of urine. It is this alkali in putrid urine which makes it detergent.

There are alſo other animal ſubſtances which afford it in great quantity, when they are heated and ſcorched to a black coal in cloſe veſſels. It is got moſt plentifully in this way from the horny parts and the bones. Formerly the horns of the hart were preferred to other animal ſubſtances, and the volatile alkali which they afforded was called ſalt of hartſhorn. There is ſtill a volatile alkali ſo called in our ſhops, but it is prepared from bones of all kinds that can be collected. They are put into large diſtilling veſſels of iron, and expoſed to an increaſing heat, until all their volatile parts are extracted; and among theſe is found a volatile alkali, which is preciſely of the ſame nature with the volatile alkali of hart's horns. It has a very fœtid odour, proceeding from a burnt and volatile oil, which ariſes from theſe ſubſtances during the proceſs, and which cannot afterwards be completely ſeparated from the alkali without too much expence.

The beſt way to have this alkali free from ſuch offenſive impurities, is to extract it from a compound ſalt called ſal ammoniac, the proceſs for which is to be ſhewn hereafter. Thus prepared, it is called ſpirit of ſal ammoniac, or ſal ammoniacum volatile, or ſal volatile.

The other names that have been given to it are *ammonia*, in the London and Edinburgh Pharmacopœias, *ſpirit* or *ſalt of urine*, *ſpirit* or *ſalt of hartſhorn*, *volatile alkali*. The French now give it the name of *ammoniac*.

In the courſe of our examination of chemical bodies, we ſhall have frequent occaſion to ſpeak of this alkali, and will alſo be able to form a more diſtinct notion of its origin and nature.

It is much uſed in medicine, as a powerful and grateful ſtimulant

and diaphoretic of quick operation, and is reckoned an antidote againſt the bites of venomous animals. It is alſo employed in ſome of the arts, as in the manufacture of ſal ammoniac, in the manufacture of alum, and in cleanſing wool, ſilk, and other ſubſtances, to prepare them for receiving colours in the art of dying.

GENUS II.

ACID SALTS.

YOU muſt next be made acquainted with the acids, ſo denominated from their taſte.

Of the ſalts belonging to this order, there are a much greater number than of the alkalis. But there are three, which, on account of the eminent and diſtinct manner in which they poſſeſs the diſtinctive properties of acids, claim our chief attention, and greatly aſſiſt us in inveſtigating the properties of the reſt.

Theſe three have commonly been diſtinguiſhed by the title of FOSSIL ACIDS, and are now ſpecified by the names of the SULPHURIC or VITRIOLIC, the NITRIC, and the MURIATIC ACIDS. Their common properties are theſe:

1ſt, They have a very ſtrong attraction for water, and being at the ſame time very volatile, a conſequence of theſe qualities is, that the only form in which we can have them for uſe, is that of watery fluids, or united with water, which repreſſes their volatility, and renders them manageable.

We cannot ſeparate the water entirely from them by evaporation, or otherwiſe: They retain it too ſtrongly, and are themſelves too volatile to admit of this. But when they are much weakened or diluted with water, we can ſeparate a part of this water by diſtillation,

and thus ſtrengthen, or, as we ſay, *concentrate* the acid to a certain degree; and in this ſtrong ſtate they are commonly kept for uſe. I ſhall, therefore, deſcribe them as they appear when in this ſtrong ſtate, or as united with as little water as poſſible.

In this ſtate they have a powerful attraction for more water. They attract it from the air more quickly, and in greater quantity than the vegetable fixed alkali does: And when mixed with water in its ordinary ſtate, they unite with it rapidly with violence, and produce heat. Upon this account, it has been thought the more extraordinary, that, when mixed with ice or frozen water, they produce a conſiderable degree of cold. But I formerly gave an explication of this phenomenon, as depending on abſorption of heat.

Although theſe acids, therefore, are always exhibited and uſed in the form of fluids, we may conſider their fluidity as depending upon the water that is mixed with them, and conclude that they are ſalts diſſolved in water: For, that the ſaline matter which they contain is capable of appearing in the form of ſolid ſalts, is plain from ſeveral facts.

One of theſe is the ſolid form which the ſulphuric acid actually aſſumes upon ſome occaſions. The whole of it congeals into a maſs reſembling ice, which, on account of its appearance, is called Glacial Vitriolic Acid.

The nitric acid alſo, when expoſed in its ſtrongeſt ſtate, to a cold of 40° of Fahrenheit's ſcale, aſſumed, in part, a ſolid form. A conſiderable part of it cryſtallized into ſpicular cryſtals.

With reſpect to the muriatic acid, the ſame experiment has not been made with it. But it is probable that it alſo would cryſtallize in the ſame degree of cold, or in a greater *.

At any rate, there is an eaſy method by which we can reduce theſe acids to a ſolid form, which ſerves to ſhew that they are capable of ſolidity. The method I mean is to combine them with the fixed alkalis. They are diſpoſed to unite readily and ſtrongly with the al-

* It does cryſtallize. EDITOR.

kalis, and form with them compound ſalts, which can eaſily be ſeparated from water, and made to appear in a ſolid form, as we ſhall ſee hereafter.

2dly, A ſecond quality which the acids are poſſeſſed of in common is, that, like the alkalis, they change the colour of infuſions of the purple or blue flowers of vegetables; but whereas the alkalis change them to a green, or aboliſh the red and deepen the blue, the acids, on the contrary, change them into bright red, or into a ſcarlet, and an extremely ſmall quantity is ſufficient. And this effect they produce upon the ſame juices, although they have been previouſly tinged green or blue by an alkali: while the alkali, on the other hand, will produce a green colour in thoſe juices, although they have been previouſly tinged red by an acid. We can, in this way, change the colour of the ſame infuſion to a red or green alternately, for a number of times, by adding the one or the other kind of ſalt.

The chemiſts are, on many occaſions, much intereſted in aſcertaining the preſence of an alkali or an acid, and keep in readineſs phials of coloured tinctures of vegetables. They alſo uſe for this purpoſe ſlips of paper ſtained with thoſe juices, called TEST-PAPERS. Theſe teſts muſt be carefully kept from the action of the air and the light.

Different tinctures have different degrees of ſenſibility to acids and alkalis. The tincture or dye called archil, cudbear, or litmus, is the moſt ſenſible to acids. The juice of the ſcrapings of red cabbage and radiſh is alſo very ſenſible to acids. March violet, convolvulus, viola tricolor, and cyanus, are leſs affected. Mallow flowers, march violet, convolvulus, and alkanet, are much more ſenſible to the alkalis.

3dly, A third quality of the acids in general is, that they readily unite with any of the alkalis, and if theſe are in their ordinary ſtate, a violent effervеſcence is produced. This effervеſcence was formerly reckoned a criterion of acids or alkalis.

This mixture is a true chemical combination. All the diſtinguiſhing qualities of both ingredients are gone. The acid or the alkaline

taſte is no longer perceived; and one different from both is induced. The mixture makes no change in the colour of vegetable infuſions. This gives us the firſt employment for our teſts and teſt-papers. By theſe we diſcover the exact ſaturation of the ingredients, which is another proof of the chemical combination. The great attraction for water is gone; and the compound is eaſily ſeparated from it by cryſtallization. The great volatility of the ingredients is alſo repreſſed; and this, in one inſtance, in a moſt unaccountable degree. The marine acid, and the volatile alkali, in their utmoſt ſtate of purity, are ſo volatile that we cannot condenſe their vapours. Yet theſe vapours, when united, inſtantly form a dry powder, which requires a very conſiderable heat, approaching to ignition, to convert it into vapour *.

4*thly*, and *laſtly*, The acids are powerful ſolvents of a number of different ſubſtances, as chalk, limeſtone, marble, and metals; and they alſo ſhew a ſolvent and deſtructive power, when applied to animal and vegetable ſubſtances. Taken internally, in their ſtrong ſtate, they would prove inſtant poiſon. Applied externally, they immediately occaſion ſharp and violent pain, and very ſoon deſtroy the part, as burning coal would do. All this is to be underſtood of them only in their ſtrong ſtate; for if they be very greatly diluted, they are not corroſive; and their moſt general effect is then to prevent or retard fermentation and putrefaction. They are often taken in this diluted ſtate as medicines, and are cooling and aſtringent, and impreſs upon the tongue, in a very diſtinct and lively manner, the particular taſte which their name implies.

Theſe therefore are the qualities which theſe acids have in common. We ſhall now conſider the peculiar nature of each of them ſeparately.

* This almoſt overturns all our mechanical explanations of chemical phenomena. EDITOR.

VITRIOLIC ACID.

The vitriolic acid has this name from vitriol, a compound in which it is contained, and from which it was formerly extracted. Now, all that is brought to the market is prepared from ſulphur; for which reaſon, and others, the French chemiſts have lately propoſed that it ſhall be named *Acide Sulfurique*, Sulphuric Acid. And it muſt be acknowledged that this is a more proper name than the other; for even the acid which we find in vitriol has its origin from ſulphur.

When pure, it is a colourleſs heavy fluid, in ſpecific gravity to water as $18\frac{1}{2}$ to 10. This great weight gives it ſome appearance of ſluggiſhneſs like oil; for which reaſon, it has been common to call it oil of vitriol.

It is alſo much leſs volatile than water. It therefore does not emit any ſenſible vapours or effluvia, in the ordinary heats of the air, nor has it the ſmalleſt perceptible odour.

It is congealable by cold, and the more eaſily as it is ſtronger. It is then called Glacial Oil of Vitriol. When of the gravity 1.78, it congeals at 32°, and requires 45° of Fahrenheit to thaw it. This ſhews that it is not a ſimple congelation, but, in ſome meaſure, alſo a cryſtallization. If undiſturbed, it will remain fluid below 32°, and freezes and emits heat when ſtirred. (*Phil. Tranſ.* 1787, Part 2d.)

When very ſtrong, it ſhews the powerful attraction it has for water, by the violence and rapidity with which it mixes with that fluid; in mixing with which it grows very hot, and contracts greatly in bulk. The heat produced, by ſuddenly mixing ſtrong vitriolic acid and water, will ſplit any thick glaſs veſſel. The mixture ſhould, therefore, be made gradually, by ſmall additions, and agitating the mixture after each addition; otherwiſe the heavy acid will collect below; and when you agitate, you will mix a great deal, and produce much heat.

The bleachers often pour the acid careleſsly even into hot water, and it breaks out into ſteam, and ſcatters the acid about on the goods.

The contraction of bulk is very great: 28 meaſures of water, mixed with four of acid, make only 29 meaſures of mixture, inſtead of 32.

Its attraction for water likewiſe appears from the quickneſs with which it attracts it from the air, and the quantity it will attract. Dr. Gould, Profeſſor at Oxford, found that three drachms attracted more than a drachm of water in one day, and in 56 days, $6\frac{1}{2}$ drachms; the quantity which it attracted always diminiſhing. Newman tells us that one ounce of it in a year attracted $6\frac{1}{4}$ ounces of water.

When this acid is diluted, or mixed with too much water, it can be ſtrengthened, or the ſuperfluous water ſeparated, by diſtillation in the retort and receiver. If the quantity of water with which it is diluted be conſiderable, the firſt vapours that ariſe are pure watery ones, or little elſe; and it does not require much more heat to raiſe it than water. But, as the operation goes on, the water that remains, in proportion as the quantity is diminiſhed, is more ſtrongly retained by the acid, and a ſtronger heat is required to continue the diſtillation, until it riſe to near the 600th degree: 546, or 550, may be accounted the boiling point of the ſtrongeſt vitriolic acid that can be made. The vapour too that ariſes during the progreſs of this operation, is found to contain the larger proportion of acid, the farther we advance in the diſtillation, until at laſt what diſtils is as ſtrong as what remains in the retort; the ſmall quantity of water which it retains being ſo ſtrongly attracted by the acid, that they cannot be ſeparated from one another by heat, but ariſe in equal proportions.

I may notice, by the way, that this operation, by which we clear a fluid of a volatile ingredient, which we conſider as diluting it, is called Concentration. When we clear a volatile fluid of a more fixed ingredient, we are not ſaid to concentrate it, but to rectify it. We concentrate vitriolic acid, but we rectify vinous ſpirits.

This acid, as firſt produced in the manufactory of it, is always much diluted with water; and it muſt be concentrated by diſtillation, before it can be carried to the market, or delivered to thoſe who make uſe of it. And the concentration is always carried on ſo far as to bring it up to the ſpecific gravity I mentioned, viz. 1 8½ to 10.

Such are the properties of this acid with regard to water. But, before we quit it, I find it neceſſary to take ſome notice of its relation to inflammable ſubſtances, the effects of which upon this, as well as ſome other acid ſalts, muſt be known before we can underſtand the proceſſes by which ſome of the compound ſalts are decompounded.

The vitriolic acid acts powerfully on moſt inflammable ſubſtances. If ſome olive oil, for example, be poured on vitriolic acid in a glaſs, heat is generally produced, and the touching parts immediately become of a very dark coffee colour. Fœtid ſteams ſoon ariſe, as the mixture goes on, and become at laſt very offenſive and ſuffocating. The mixture becomes thick and black, like tar. If we mix the acid in the ſame way with oil of turpentine, the mixture is more remarkable, with violent heat, and burſts of ſuffocating ſteams, and the viſcid mixture foams and froths over the brim of the veſſel. The oil acquires a very particular ſmell, and ſome of the ſpongy matter becomes hard and charred.

One phenomenon obſervable in all theſe mixtures of vitriolic acid with common inflammable ſubſtances, is the very dark or black colour produced. This occaſions the brown colour which this acid acquires when kept in a negligent manner. The acid of vitriol that is to be found in the ſhops, is often of a dark brown or black colour, although, when freſh or new, it is perfectly colourleſs and tranſparent, and if kept in cloſe veſſels with a glaſs ſtopper will remain ſo; but if any duſt be allowed to get into it, or if the phial be ſtopped with a cork or other vegetable inflammable matter, the acid diſſolves and corrodes it, and acquires more or leſs of a brown tinge or even black colour. The way to reſtore a vitriolic acid thus tainted, is to diſtil off a ſmall

part of it in a retort and receiver, or boil it in a matraſs under the vent. When we do this, a ſmall part of the acid ariſes in the form of volatile ſteams, which have the ſuffocating odour of burning ſulphur; the reſt becomes clear, and free from colour or ſmell.

When we have had the curioſity to apply heat to ſuch mixtures as theſe, and to ſubject them to diſtillation, we have obtained ſeveral products:

1ſt, A great deal of the acid ariſes in that volatile and ſuffocating ſtate which I juſt now mentioned, in which ſtate we call it the ſulphurous acid; and along with it the water which was combined with it at firſt, and alſo a quantity which is contained in the compoſition of the oil.

2dly, After this water and ſuffocating ſulphurous acid have ariſen in conſiderable quantity, vapours next ariſe, which are vapours of perfect ſulphur or brimſtone; a part of the acid being changed into this form by the action of the inflammable matter and it on one another. We alſo frequently get a dry black powder, which, on examination, is a pure charcoal. This riſes, or at leaſt is brought off from the black mixture with a very moderate heat, and appears to have been merely floating in it, having probably been the cauſe of its blackneſs.

A great variety of other inflammable ſubſtances may be employed, as well as theſe oils, for producing theſe changes on the vitriolic acid. I ſhall juſt mention two inſtances, which exhibit claſſes of appearances ſomewhat different, and lead the philoſophical chemiſt to a variety of different concluſions.

The firſt is a very ſimple one. Dr. Stahl, obſerving the ſulphurous fumes obtained in the proceſs which I have juſt now deſcribed, and the genuine ſulphur ſometimes produced, and that this ſulphur did not appear till towards the end of the proceſs, when the matter in the retort was of a burning heat, wiſhed to force the acid to bear this heat from the beginning, while it acted on inflammable bodies. For this purpoſe, he took the acid as it is found combined with another ſubſtance

in the form of a ſalt. He choſe alum, of which it is an ingredient. He mixed dry alum with powdered charcoal. Applying a very great heat, he ſublimed a quantity of pure ſulphur. A prodigious volume of air was produced, which could not be condenſed by cold, and would have burſt the veſſels, if not allowed to eſcape.

The other example is the diſtillation of the acid from dry vegetable matter, ſuch as ſugar or ſtraw. If this laſt be put into the ſtrongeſt ſulphuric acid, it is *immediately* blackened, and after ſome time, diſſolved into a dark brown or black pulp. No ſmell or ſuffocating fumes are produced in the cold. If we now dilute with water, a conſiderable quantity of impalpable black powder is thrown down after a long ſtanding. This, when examined, is very pure charcoal or carbone. It is remarkable, that this great addition of water to the ſtrong acid produces but a moderate heat; it produces, however, a diſtinct ſmell of vinegar. If we now proceed to diſtillation, we obtain real vinegar from it, and a much ſmaller proportion of the ſulphurous acid and brimſtone than in the proceſs with oil.

We ſhall return to this proceſs, when we ſhall have acquired a clear knowledge of the agents which are concerned in theſe changes.

In theſe proceſſes, therefore, the acid undergoes a moſt remarkable transformation; for that it is a part of the acid which is thus tranſformed into ſulphur, cannot be doubted a moment. When we examine this ſulphur, and make experiments with it, we find that it has every quality of common ſulphur, and among the reſt, that of being convertible into vitriolic acid by inflammation in the free air.

I have been ſomewhat minute in the account given of this action of vitriolic acid on inflammable ſubſtances, becauſe it may be affirmed that it was the obſervation of theſe phenomena by the celebrated Stahl that firſt introduced any thing like philoſophical diſcuſſion into chemiſtry. The vague and obſcure notions of Geber, Becher, and others, about their ſulphur and ſulphuric principle, do not deſerve ſuch a name. But Dr. Stahl founded, on the obſervations which I

have now related to you, a body or ſyſtem of very preciſe and perſpicuous doctrines, whoſe influence connected all the great and important phenomena in chemiſtry. Obſerving that the mixture of vitriolic acid with *every* inflammable ſubſtance produced the ſame ſulphur, and that the ſubſtance *was no longer inflammable*, he inferred, with great propriety, that all inflammables imparted one and the ſame ſubſtance to the acid. Moreover, as thoſe bodies reſembled only in inflammability, and as none but inflammables produced ſulphur with the acid, he as properly concluded that the thing thus imparted was the cauſe or principle of their inflammability. He called it the PHLOGISTON. "*Materiam et principium ignis* (ſays he) *ego phlogiſton appel-*
"*lare incepi; nempe, primum, igniſcibile, inflammabile, directè atque emi-*
"*nenter ad calorem ſuſcipiendum habile, principium.*" Adding this to any body that attracts it, renders it inflammable, and taking it away, renders it uninflammable. Inflammation is its diſſipation; therefore the remains are uninflammable. It may alſo be taken away by another body which attracts it more ſtrongly; and this transference is not accompanied by inflammation. It is thus that vitriolic acid takes it from charcoal.

Such is the famous doctrine of Stahl; and its immediate and intereſting conſequences explain all the operations of metallurgy. It was received with great applauſe; and Germany became the great ſchool of chemiſtry.

Such being the ſtate of our ſcience at the beginning of the century, all the chemiſts were accuſtomed to conceive and explain theſe phenomena according to the doctrine of Dr. Stahl, by ſuppoſing the volatile ſuffocating acid, which ariſes before the ſulphur, to be a part of the vitriolic acid, combined with a certain proportion of their ſuppoſed phlogiſton or principle of inflammability; and the ſulphur, which is formed afterwards, to be the reſt of the acid, combined more perfectly, or ſaturated with a larger proportion of the ſame principle. The effect of this is to make it aſſume the form of an inflammable

ſubſtance, namely ſulphur, which ſulphur may be inflamed and burned, ſo as to diſſipate its principle of inflammability, and thus to give us the acid again in its pure ſtate. This, I ſay, was the manner in which the chemiſts were accuſtomed, until lately, to underſtand the facts I have narrated.

But a great number of important diſcoveries which have been made of late, have occaſioned theſe facts to be now viewed in a different light. It has been diſcovered, that while the vitriolic acid undergoes theſe changes into ſuffocating acid and ſulphur, there is extricated from the materials a very great quantity of air, or elaſtic aërial matter; and, on the other hand, when ſulphur is changed again into acid by inflammation, a quantity of air, without which it cannot be burned, is abſorbed, ſo as to enter into the compoſition of the acid, and make up a part of it; the quantity of vitriolic acid obtained being, in fact, of much greater weight than the quantity of the ſulphur burnt, nearly $2\frac{1}{2}$.

It is therefore concluded now, that the vitriolic acid is a compound of ſulphur and air, and that, when the acid is mixed with inflammable ſubſtances, and, by being diſtilled with theſe, is converted into ſulphurous acid and ſulphur, theſe changes are not produced by the addition of any matter which it receives from the inflammable body; but that, on the contrary, by a decompoſition which it ſuffers, a great quantity of its air being ſeparated from it by the ſuperior attraction of the inflammable body for that air; and that part of the acid which has loſt a *portion only* of its air, becomes, by its redundancy in ſulphur, ſulphurous acid, or volatile ſuffocating vitriolic acid; while that portion which loſes the whole of its air becomes perfect ſulphur.

On the other hand, when ſulphur is ſet on fire and made to burn, and by inflammation is converted into acid, it unites with a part of the air which is in contact with its flame, and which is neceſſary to the inflammation; and by uniting with, or attracting to itſelf, this air, it becomes vitriolic acid again, or a compound of ſulphur and air.

That a quantity of air is abſorbed by ſulphur, when converted into acid by inflammation, is now acknowledged by every chemiſt. The moſt direct proof of this is had by burning ſulphur in a known quantity of air, in the following manner:—In a ciſtern of water, or mercury, ſet up a ſtand, riſing ſome inches above the water, and on the top of it put a ſmall quantity of ſulphur; have ready a glaſs jar, and having kindled the ſulphur, inſtantly cover it with the jar, whoſe mouth muſt dip an inch or two in the water. The heat will expand the air a little at firſt, and the water will be preſſed down; but by and by the water will riſe in the jar, and, when the brimſtone ceaſes to burn, and the whole has grown cold, the water will have riſen conſiderably in the jar; that is, air has been abſorbed. By attending to all the circumſtances which affect the reſult, it is found that the weight of the air abſorbed is equal to that gained by the ſulphur conſumed by inflammation.

It is alſo completely proved that a great quantity of air is extricated, when we convert the acid into ſulphur by the action of inflammable ſubſtances. Of this you had an example in the proceſs lately ſhewn with alum and charcoal. For, unleſs an exit be allowed, the veſſels will be burſt: And if the vapour be collected in another veſſel, it will be found aërial, that is, incondenſable by cold. It is, therefore, no longer diſputed that the acid is a compound ſubſtance, having air, or the baſis of air, for one of its component parts. But ſome of the chemiſts ſtill maintain that ſulphur alſo is a compounded ſubſtance, as well as the acid; that there is a ſpecific matter in ſulphur, which matter, when combined with air, forms vitriolic acid, and when combined with the principle of inflammability, forms ſulphur; and that while this ſpecific matter unites with air, it loſes the principle of inflammability; and that when it unites with the principle of inflammability, it loſes its air. This opinion, however, is not ſupported by direct experiments as the other is; and until thoſe who maintain it can ſhew this ſpecific matter in a ſeparate ſtate, and make us ac-

quainted with its nature and properties, there is reaſon to ſuſpect that their opinion proceeds rather from the old habits of thinking, than that it is founded on fact. They think it is neceſſary to ſuppoſe a principle of inflammability in ſulphur, in order to account for the light and heat which it gives in burning. But, by the French theory, this light and heat is ſuppoſed to proceed from the latent and ſpecific heat, the *calorique*, as they call it, which is ſeparated from the air, while this air is condenſed by the attraction of the ſulphur.

From the whole of the facts, it is manifeſt that ſulphur and the vitriolic acid are very nearly allied. Each of them is eaſily convertible into the other, and yet their properties and powers are very different. The acid is extremely acrid and corroſive with reſpect to animal and vegetable ſubſtances: Sulphur, on the contrary, is mild and harmleſs. The acid has a ſtrong attraction for water: Sulphur has not the leaſt. The acid mixed with alkalis unites quickly and violently, and is ſtrongly combined, and difficultly ſeparated: Sulphur added to alkalis does not exhibit any ſuch phenomena. Sulphur, however, has a conſiderable degree of attraction for alkaline ſalts, and is diſpoſed to unite with them. To ſee this, however, and make them actually unite, it is not enough that we mix them together in the form of dry powders. In this caſe, as in moſt others, it is neceſſary that one of the bodies, the alkali or the ſulphur, be fluid, before they will act on one another or unite. But this fluidity may be obtained in two ways, either by uſing water to diſſolve the alkali, or by applying heat to melt the ſulphur. In the language of chemiſts, the one is called the dry way, and the other the humid.

1. Sulphur, in the form of a dry powder, being mixed with an equal weight of vegetable alkali, muſt be expoſed in a crucible to a heat gradually increaſed. This melts the ſulphur, which then unites with the alkali, forming a compound fuſible in the heat, and ſoluble in water. When it cools, its colour greatly reſembles that of liver.

Hence it has been called LIVER OF SULPHUR, HEPAR SULPHURIS. This is the compound prepared *viâ ſiccâ.*

2. A ſtrong ſolution in water of vegetable fixed alkali in its pureſt active ſtate, will diſſolve powdered ſulphur, with the aſſiſtance of a moderate heat. This is the preparation of liver of ſulphur *viâ humidâ.*

This combination of ſulphur and alkali emits a moſt diſagreeable ſmell, thought to reſemble that of rotten eggs. But it has nothing putrid or cadaverous. It perfectly reſembles what you perceive in the waſhings or ſcourings of a fowling piece; and this ſmell is really occaſioned by *hepar ſulphuris.*

The ſulphur, when thus combined with an alkali, by either of theſe proceſſes, and diſſolved in water, ſhews a much greater diſpoſition to unite with air, and to be changed into an acid by the action of the air, than when it is pure. We need only to expoſe it to the air of the atmoſphere for a few weeks. Thus, by an imperceptible action of the air upon it, the ſulphur is gradually changed into acid, which however remains combined with the alkali; and we have at laſt a compound ſalt in place of the *hepar ſulphuris* which we expoſed.

This converſion of *hepar ſulphuris*, a deep coloured, ſtrong ſmelling compound, into a pure white inodorous ſalt, by the ſimple expoſition to the air, is a curious fact, and naturally excited the attention of the chemiſts. When the experiment is made in cloſed veſſels, that is, in a veſſel ſet under a jar inverted in a ciſtern of water, we diſcover that the change is effected, or at leaſt is accompanied, by the abſorption of a quantity of air; for the water of the ciſtern will gradually riſe into the jar. We ſhall find that about one fourth part of the air incloſed with the *hepar ſulphuris* can be thus abſorbed. Whatever quantity of hepar is thus expoſed to a given quantity of air, no more air can be abſorbed, and the quantity of ſalt produced is thus limited.

We learn by theſe experiments that the air of our atmoſphere is not homogeneous; that there is only a certain part of it diſpoſed to

unite with ſulphur, a little more than the fourth part. The reſt has not the leaſt diſpoſition to unite with ſulphur, either when expoſed to it in the form of *hepar ſulphuris*, or when we attempt to fire the ſulphur in it. We cannot either inflame the ſulphur, or change it into an acid, or perceive that any of the air is abſorbed.

We owe this moſt important experiment, and the concluſion deduced from it, to Mr. Scheele, of Koping, in Sweden, one of the moſt penetrating, judicious, and accurate chemiſts of the preſent age. It is to be ſeen in detail in his Publications on Air and Fire, and in the Memoirs of the Academy of Stockholm.

Much about the ſame time, and without any knowledge of the other, Dr. Prieſtley had diſcovered this elaſtic fluid, which appeared to Dr. Scheele as a reſiduum of atmoſpheric air. The train of Dr. Prieſtley's experiments led him to form a very different opinion of it. He conceived it to be atmoſpheric air combined and ſaturated with the principle of inflammability or phlogiſton, and therefore he called it *phlogiſticated air*. Scheele called it *foul air*, which we ſhould tranſlate, not tainted, but putrid, corrupted. It is now named by the German chemiſts *ſtick-ſtoff*, i. e. ſuffocating or choking air. When he learned Scheele's method of obtaining it, he was confirmed in his opinion, and ſaid that the phlogiſton, which had quitted the vitriolic acid, had united with air. But however ſpecious and perſpicuous this explanation is, and although the opinion was adopted by the moſt intelligent chemiſts of Europe, it is now found to be a miſtake. The ſpecific gravity of this phlogiſticated air was aſcertained by ſeveral chemiſts; and when this was compared with the diminution of bulk, it was obſerved that the abſolute weight of the air had been diminiſhed as much as its bulk. This ſeems incompatible with the notion of the remainder being a compound of the whole original air and phlogiſton. Few chemiſts could admit phlogiſton to be a ſubſtance which diminiſhes the weight of the ſubſtances with which it unites. Moreover, experiments were afterwards made, which com-

pletely refuted Dr. Prieſtley's opinion. Dr. Prieſtley and Dr. Scheele had diſcovered a method of purifying air from every thing that could be ſuppoſed phlogiſton, and Dr. Prieſtley therefore called it *dephlogiſticated air*. It was indeed his greateſt diſcovery, and has produced a total and extenſive change in the chief doctrines of chemiſtry, as you will very ſoon ſee. Now, if this experiment of Scheele's be made with dephlogiſticated air, that is, if the jar in which the *hepar ſulphuris* is expoſed, be filled with this inſtead of atmoſpheric air, the ſeparation of the ſulphur and alkali is effected with great rapidity, and the dephlogiſticated air is *completely* abſorbed, and diſappears. Here there is no longer a place for doubt; and it is perfectly agreeable to Scheele's view of the matter. There is no foul air in the beginning, and therefore none can remain.

Theſe experiments ſucceed exactly in the ſame manner when we make uſe of the foſſil alkali in its pureſt ſtate. And ſulphur is alſo capable of being joined to the volatile alkali, but by a very different proceſs, of which we ſhall ſpeak hereafter.

From the experiments you have now ſeen, you have gotten ſome knowledge of the nature of ſulphur, and of the origin of the vitriolic acid, and you ſee the propriety of the new names. *Sulphuric* denotes its origin, and *ſulphurous* (ſulphuroſus) denotes a redundancy of ſulphur in the volatile ſuffocating acid.

Of this laſt, it is proper to remark that it is much leſs eminently acid, and when combined with ſulphur to the utmoſt, has little ſourneſs of taſte. Its attraction for water, and for alkalis, is alſo prodigiouſly diminiſhed. It even ſeems to unite with water reluctantly; and expoſure to air takes away the peculiar ſmell, and gives us ſimple ſulphuric acid. There are even ſome curious experiments, where the ſuffocating vapour is made to depoſit the ſulphur it contains in a ſnow-white dry powder. The weakeſt acids diſlodge it from an alkali.

The change of ſulphur into acid is performed by the chemical

artiſts as a manufacture. It may be effected by different proceſſes, but only one of them produces the acid in its moſt perfect and moſt active ſtate. This proceſs conſiſts in an inflammation of the ſulphur. A proceſs for this purpoſe was preſcribed formerly in our Pharmacopœias, under the title of *Spiritus Sulphuris per Campanam*, and had this name from the apparatus employed, which was a large glaſs bell hung over a wide diſh, in the middle of which ſtood a ſmall veſſel with burning ſulphur. By this proceſs, however, a ſmall quantity only of the acid was obtained. By far the greater part aroſe from the flame of the ſulphur in the form of the ſulphurous acid, which was very difficultly condenſed, and when condenſed, afforded an acid liquor, which continued to have the ſame ſuffocating odour, and was very different in ſeveral reſpects from the ſtrong vitriolic acid.

It is evident that this proceeded from an imperfect inflammation, and incomplete change. Proceſſes have ſince been contrived, by which the inflammation is made more complete, and the ſulphur is changed into a large quantity of perfect vitriolic acid. I ſhall mention the manner of doing this hereafter.

This much is ſufficient at preſent with reſpect to ſulphur, of which it was neceſſary to take ſome notice, on account of its connection with vitriolic acid. We are to take farther notice of it hereafter, in treating of inflammables.

Let us next turn our attention to the natural hiſtory of this acid. Some time ago, it was very generally ſuppoſed among the chemiſts that this acid abounds in the air, or at leaſt that there is ſome of it always mixed with common air. It was called the *Acidum vagum Aëreum*. Nay, ſome of the moſt eminent chemiſts conceived it to be the primitive acid, and called it *Acidum Primigenium*, affirming that it made the baſis of all acids. But the experiments to prove theſe points are not ſatisfactory, and there are others which are incompatible with them.

The place in which this acid is in reality found, is the interior parts of the earth. Sometimes, if we may credit obſervers, it is found combined with water alone, in ſome mineral waters. But this I think very doubtful. The bad air, or vapour, by which miners have been ſometimes ſuffocated, has alſo been ſuppoſed by ſome to be vitriolic acid, on account of the ſteams of burning ſulphur being alſo ſuffocating. But all accounts of thoſe vapours ſhew them to be a very different matter; of which hereafter.

Upon the whole, we have no clear evidence that this acid ever occurs in a ſeparate ſtate, except in the ſteams and ſmoke of volcanoes and volcanic ground; which ſteams are ſaid often to have a very perceptible and ſuffocating odour of burning ſulphur. But it is found in great quantity combined with other matter; as in gypſum combined with lime, in bitter purging ſalt combined with magneſia, in alum and aluminous ſtones or clays, and in vitriol, green, blue, and white; the origin of all which, and the proceſſes for obtaining the acid from them, will be conſidered as we proceed.

Uſes.——This acid is itſelf employed as a medicine, and is neceſſary in the preparation of a great number. It is uſed in the arts, in the proceſs of making aquafortis, in ſeparating copper from gold, alſo in the bleaching of linen, the tanning of leather, and the art of dying. The ſulphurous acid is uſed for whitening woollen ſtuffs and ſilks, &c. &c. and vitriolic acid has been the great inſtrument in the hands of the chemiſt for detecting other acids, by diſlodging them from the compounds in which they were contained.

SPECIES III.—NITROUS AND MURIATIC ACIDS.

The nitrous and muriatic acids derive their names from the nitre and ſea-ſalt, from which they are commonly obtained. In their ſtrongeſt ſtate, they are not ſo heavy as the vitriolic acid, and are there-

fore ſuppoſed to contain leſs ſaline matter and more water. The ſalt which they contain appears to be of itſelf very volatile, and to have its volatility repreſſed by the water with which it is joined. This appears when we attempt to prepare thoſe acids of extraordinary ſtrength. They ariſe in the form of fumes, which cannot be condenſed, except we apply a receiver with ſome water, by which the acid particles are ſtrongly attracted and condenſed, and brought into the form of a fluid, more or leſs manageable, according to the quantity of water with which the acid is combined; if the quantity of water be moderate, it will not be ſufficient for totally repreſſing the volatility of the acid, ſome of which is always diſpoſed to fly off in vapour whenever the fluid is expoſed to the air; but if a good deal more water be added, the diſpoſition to ſmoke in that manner will be diminiſhed, until it is entirely taken away; the greater quantity of water having more power and effect in ſuppreſſing the volatility of the acid.

But it is proper to obſerve here that both of them appear ſometimes in a more volatile, ſometimes in a leſs volatile ſtate; the cauſes of which will be explained hereafter.

When they are diluted with a large quantity of water, we can diſtil off part of this water, and render the acid again moderately ſtrong. For it is very remarkable, that when they are moderately diluted only, they compoſe with the water a fluid that is rather leſs volatile than either the water or the acid particles in their ſeparate ſtate. If we therefore ſubject them to diſtillation in a largely diluted ſtate, a part of the water diſtils over at firſt by itſelf, or with very little acid, and the acid in the retort becomes ſtronger. But they can be ſtrengthened in this manner to a certain degree only, which falls conſiderably ſhort of the ſtrength to which vitriolic acid may be brought by this operation. This proceeds from their being more volatile than that acid.

What has been ſaid ſo far, therefore, is applicable to both theſe acids. But we muſt conſider more particularly the nature of each by itſelf.

NITROUS ACID.

First, therefore, the nitrous acid, when very ſtrong, ſmokes if expoſed to the air, and emits ruddy fumes, of a highly diſagreeable ſuffocating ſmell, which tinge the air in the phial in which it is kept of a deep orange colour, but perfectly tranſparent. When this vapour gets out, and mixes with the air, it forms a denſe cloud of ruddy miſt, ſometimes as red as blood. The acid may be cleared of theſe fumes by diſtilling off a ſmall quantity, and is now called NITRIC ACID. What remains is equally, perhaps more acid than before, but does not ſmoke; yet if this acid be expoſed to the light of the ſun, in a phial cloſe ſhut by a glaſs ſtopper, it immediately begins to tinge the air above it, and to produce the ſame fumes.

The moſt eminent and diſtinguiſhing property of this acid is the ſtrong action which it exhibits on inflammable bodies, and the violent effects which it produces, when mixed with a number of thoſe ſubſtances. They are among the moſt ſurpriſing phenomena that chemiſtry exhibits. You have a good example in its action on the aromatic oils. If one of theſe, ſuch as oil of caraways, or even oil of turpentine, be put in a ſmall diſh or cup, and about two ounces of the ſtrongeſt ſmoking nitrous acid (uſually called Glauber's ſpirit of nitre) be poured into the middle of it, the mixture immediately grows hot, ſmokes, boils, or frothes up, and burſts out into a bright flame. The experiment is attended with ſome riſk of being beſpattered with the burning ſtuff, and ſhould therefore be performed in a particular way. Some little circumſtances muſt alſo be attended to for inſuring ſucceſs. Let the oil be in a diſh, in form of a cup, and put the acid into a three-ounce phial, of which the neck is cut off; this muſt be faſtened to the end of a long ſtick as a handle. The quantity of the acid ſhould be about half of the oil. Pour in about two thirds of it; and as ſoon as the violent boiling begins, pour in the reſt, where the boiling ſtuff

appears blackeſt and drieſt. The oil of turpentine ſhould either be thick by long keeping, or be thickened a little by ſome turpentine. Mixing about a third part of the ſtrongeſt vitriolic acid with the nitrous acid is thought to make the ſucceſs more certain.

This ſurpriſing experiment was firſt publiſhed by Dr. Hoffman, with reſpect to one kind of oil. But others have ſince ſhewn that the nitrous acid produces the ſame effect upon a great number of the oils, and even upon charcoal; when finely levigated, it fires it inſtantly; only, that with ſome of thoſe oils the mixture muſt be made in a particular manner: And, in general, the acid muſt be uſed as ſtrong as poſſible.

As the vitriolic acid is blackened, and made to yield ſuffocating vapours, by the ſmalleſt addition of inflammable ſubſtances, ſo ſuch addition to the nitric acid cauſes it to aſſume the orange colour, and give out thoſe ruddy fumes; and is now in the ſtate called NITROUS ACID.

From the late diſcoveries in chemiſtry, there is reaſon to conclude, that this power of the nitric acid to act with ſuch violence on the inflammable ſubſtances, depends on its containing a large quantity of the ſame ſort of air which makes a part of the vitriolic acid,—a particular kind of air which I ſhall ſoon make known to you. In the nitric acid it is more looſe and active than in the vitriolic acid; and there is a ſtrong attraction between this air and the inflammable ſubſtances. The nitric acid, therefore, in acting on ſome of them, produces elaſtic fluids, and violent exploſions, and very bright and rapid inflammations. All theſe particulars, in the conſtitution of the nitric acid, and their relation to the phenomena of combuſtion, will open upon us by degrees, as we proceed in our examination of chemical bodies.

This acid mixes with water, producing heat, ebullition, red fumes, and a green or blue colour, which, by adding more water, is weakened, and diſappears. This blue colour alſo appears to be owing to the action of inflammable ſubſtances; for, when it is diluted, and has

loſt its blue colour, it may be reſtored by a ſingle drop of ſpirit of wine added to ſeveral ounces of the diluted acid. N. B. This diluted acid is the *aquafortis* of the ſhops.

You may remember that, on another occaſion, I mentioned the remarkable effect of this acid on water, in the form of ice or ſnow, eſpecially the laſt. It melts them into an acid brine, and the liquefaction is accompanied with moſt intenſe cold. But I then explained the cauſe of this ſudden diſappearance of heat. It is no way inconſiſtent with the production of heat by the mixture of the acid with water. Vitriolic acid produces the ſame effects, but the cold is inconſiderable, becauſe the freezing or cryſtallizing temperature of the acid is not very low, whereas the nitric acid has never yet been congealed by cold, when moderately ſtrong.

Natural Hiſtory.——All the nitric acid we have is extracted from a compound ſalt, namely, common nitre or ſaltpetre; with the natural hiſtory of which I ſhall ſoon make you acquainted. There is alſo unqueſtionable evidence, by experiments of Mr. Kirwan, Milner, Berthollet, and others, that it may be produced by mixing air with pure volatile alkali. You will ſee, as we proceed, how this may happen, although neither of thoſe vapours contain this acid.

This acid is employed in pharmacy, in preparing a number of the articles of the materia medica, and is neceſſary or uſeful in many other arts, as parting of gold and ſilver, etching, ſcarlet dye, and others.

SPECIES III.—MURIATIC ACID.

The muriatic acid, in its ordinary ſtate, is a light yellow, or citron coloured fluid, which, being expoſed to the air, emits fumes, which do not tinge the air, but are colourleſs and tranſparent in the phial; and when allowed to exhale, and mix with the air of a chamber, they render it miſty. Theſe fumes alſo are noxious to the lungs of thoſe

who are much expoſed to them; but their ſmell is very different from that of the nitrous acid, and is thought by ſome to reſemble the ſmell of ſaffron.

Theſe are the characters of this acid in its more fixed ſtate; but I muſt not omit to inform you that it is alſo liable to great difference in the degree of its volatility. As obtained by the proceſs uſually preſcribed for preparing it ſtrong, it is of a paler colour, and ſometimes has hardly any tinge of yellow. It is then in a much more fuming ſtate, and the fumes are much more diſagreeable and ſuffocating. Yet with all theſe appearances, it is not ſo ſtrong, as an acid, as when leſs fuming. If we diſtil away the half, the fuming, or moſt volatile part, comes over, while the ſtronger and more fixed remains in the retort. This more fuming ſtate ſeems to depend upon the ſame cauſe as in the nitric acid; but we cannot demonſtrate this ſo clearly by extemporaneous experiments *.

In mixing with water, it does not produce ſo much heat as the former two foſſil acids, and its colour is only diluted and the fumes ſuppreſſed.

It has very little attraction for inflammable ſubſtances. It acts upon them but very ſlowly and weakly, never producing heat or ebullition. Its action is beſt obſerved when it is digeſted with them. It is then found to darken in its colour, and thicken in its conſiſtence.

Origin.——Margraaf obſerved that a minute quantity of it exiſts in the air; but it is found no where elſe in a ſeparate ſtate. Great abundance of it, however, is found in a ſtate of combination. Common ſalt, which (as we obſerved before) is formed in ſuch great quantities in the earth, as well as in the ſea, and one half of which is compoſed of

* I confeſs that the two caſes ſeem to me to be remarkably different. The vitriolic and nitric acids are moſt eminently acid, when they have no redundancy in their diſtinguiſhing ingredient. The muriatic acid is moſt eminently acid, when it has the greateſt proportion of its diſtinguiſhing ingredient. This anomaly ſtill remains a myſtery.

EDITOR.

foſſil alkali, has this acid for its other half; and from it this acid is always obtained.

The preceding obſervations may ſuffice for giving you a preparatory knowledge of the general characters of the mineral acids, as far as is neceſſary for underſtanding their manner of acting on certain great claſſes of chemical bodies. In the conſideration of thoſe bodies, and of the way they are affected by their union with theſe acids, we ſhall learn by degrees much more concerning the nature and conſtitution of the acids themſelves.

I obſerved before that there are many more acids than theſe: I do not propoſe, however, to take notice of them all at preſent, but will confine myſelf to the deſcription of three of them, of more frequent occurrence. They are diſtinguiſhed from the former, or mineral acids, by being much leſs powerful as acids, and leſs perfect as ſalts.

Theſe three are called,

The Acetous Acid,

The Acid of Tartar, and

The Sedative Salt, or Acid of Borax.

The acetous acid, and acid of tartar, are called the vegetable acids, on account of their being obtained from vegetables only. The acetous is got from fermentable vegetable ſubſtances, by fermenting them in a particular manner, and it ſeems to be formed by the fermentation. The acid of tartar alſo is obtained chiefly from fermented vegetable juices, but appears to exiſt in the vegetable matter before the fermentation, which does not form this acid, but only extricates it. The nature of fermentation, and the particular kinds of it by which theſe acids are obtained, ſhall be conſidered on another occaſion. At preſent we ſhall attend to the properties of theſe two acids thus obtained.

SPECIES IV.—ACETOUS ACID.

The ACETOUS ACID, or VINEGAR, is of all the acids the moſt commonly known, and from it even the foſſil acids derived their name. It reſembles them in ſeveral particulars. Like them, it is a watery fluid, and, in its original ſtate, is always unavoidably diluted with a very great quantity of water; ſo that if we compare its ſtrength with that of the foſſil acids, we ſhall find that it has hardly equal acidity to a mixture of theſe with 40 or 50 times their weight of water. It is, in fact, a mixture of the true ſaline matter and water, in the ſame manner as the foſſil acids. It may be freed from a very conſiderable portion of the water by freezing. The ice which is formed ſcarcely retains any of the acid. Having done this by a moderate froſt, the liquor which remains, and which contains almoſt the whole acid, may be expoſed to a more intenſe cold, which will ſeparate ſtill more water, and leave the acid of very great ſtrength, not leſs than ten times its former ſtrength. But this method cannot be practiſed except in the coldeſt climates. But you will learn other methods by which it may be freed from a great portion of this ſuperfluous water, and made equal in acidity to ſome of the mineral acids.

But beſides this great proportion of water in which it is diffuſed, we find it mixed with a quantity of mucilaginous and inflammable matter, and ſome other ſaline bodies beſide our acid. When we deſire to render it purer for chemical purpoſes, we ſeparate theſe impurities by diſtillation, the purer acid being ſo volatile as to riſe by a moderate heat, very little exceeding that of boiling water, while the other matters, which are not diſpoſed to riſe at this heat, remain. It is then called diſtilled vinegar. By the ſame operation too we can free it of ſome part of its water: That is, having firſt ſeparated it by diſtillation from thoſe extraneous matters which are leſs volatile, we ſe-

parate ſome of the water that dilutes it, by another diſtillation, in which the dephlegmated acid remains in the retort. By this method, the acid is pretty well ſeparated from the ſlimy, tartarous, and other fixed matters. But the ſeparation of the water, or the dephlegmation or concentration of vinegar, as it is called, can be carried only to a very imperfect length by diſtillation alone, on account of the ſmall difference in point of volatility between the acid and the water. Other ways, therefore, have been attempted, 1*ſt*, By freezing, as has been already mentioned; 2*d*, By joining ſome fixed ſubſtance, which diminiſhes its attraction for water, and fixes it ſo as it can be reduced to a dry maſs; and then it may be ſeparated by heat alone, if it be not too ſtrongly retained, or with the aſſiſtance of ſome addition, ſuch as charcoal duſt *. At preſent, I can only notice the general principle on which thoſe additions are made, namely, to force the acid to bear a conſiderable heat. But this alone would not be enough. It only comes in aid of elective attractions, which of themſelves are too weak. The particular modification of each of theſe additions not being yet familiar to you, I need not name them at preſent: They will occur in courſe.

Thus it may be made as ſtrong as the foſſil acids; for, though it is more eaſily elevated by heat applied, it is not ſo elaſtic as the nitric and muriatic acids. In this very concentrated ſtate, the acetous acid cryſtallizes, and its vapours are inflammable, as alſo the acid itſelf, if made pretty warm. This operation, however, is not neceſſary to prepare the acid for the common uſes to which it is applied. For com-

* Mr. Lowitz, an eminent chemiſt at Peterſburg, found that charcoal acts on acetous acid, ſo as to make it reſiſt a heat ſomewhat exceeding that of boiling water. Therefore, by managing the diſtillation with great care, all the water can be expelled, and what remains in the retort is the charcoal, combined with the acetous acid. This ſtate of things muſt be indicated by the ceſſation of drops from the neck of the retort. The receiver muſt now be changed, and the heat raiſed a few degrees. The acid will now come over in the moſt fragrant, pure, and concentrated ſtate that can be produced.

EDITOR.

mon uſes, it is only purified by diſtillation, and it is then known by the name of *Diſtilled Vinegar*. It reſembles the foſſil acids by effreveſcing with alkalis in their ordinary ſtate, and by uniting with them, and with abſorbent earths, and ſome metals, though weakly. Like thoſe acids, it changes vegetable colours to red, but it has little effect upon inflammable bodies, though it may be combined with them, nor is it ſo acrid and corroſive, even when equal in ſtrength.

But there is another remarkable particular by which it differs from the foſſil acids, viz. by being eaſily deſtructible by the action of heat, if the heat to which it is expoſed ever riſes to the point of ignition. We may obſerve the effects of this heat upon it, by joining to it ſome fixed ſubſtance that can retain it, and repreſs its volatility with proper force, ſuch as a fixed alkali. If it be united to one of theſe, and the compound expoſed to heat in cloſe veſſels, as ſoon as the compound ſalt approaches to a red heat, the acid begins to be totally deſtroyed. The principles of it are diſarranged, and made to enter into new combinations with one another, ſo that we never can recover it again. It is ſcorched, burned, and deſtroyed by the heat, and converted into fœtid, watery, and oily ſteams, and a black coaly matter adheres to the alkali.

The ſame change is produced by fire on all vegetable matter in general; and as all other vegetable matter is inflammable, ſo is alſo this acid.

SPECIES V.—ACID OF TARTAR.

The other ſpecies of vegetable acid, which is known by the name of the *acid of tartar*, or the *tartarous acid*, is ſtill more groſs, and farther removed from the nature of the more pure and perfect ſalts. For although we can reduce it to a dry ſtate, and cryſtallize it, it is neither a fuſible nor a volatile ſubſtance. I mean that, except a watery

fuſion, which the cryſtals can undergo, it is not capable of being melted, or converted into vapour by heat, like moſt other ſalts, ſo as to reſume its natural appearance and properties, when it is cooled. When heat is applied to it, no ſooner does the heat riſe above that of boiling water, than this acid begins to be ſcorched and burned by the heat acting on it, as happens to vegetable matter in general; and as the heat is increaſed, fœtid, oily, and ſooty ſteams ariſe from it, which may be ſet on fire. A black coal remains, which may be burned to aſhes, and thus the acid is totally loſt and deſtroyed; that combination of its principles upon which its properties depend, being now ſo completely undone, that it never can be reſtored again.

Water, applied to this acid, diſſolves it eaſily and in great quantity. The ſolution, when very ſtrong, is thick like a ſyrup, and has an agreeable acid taſte, like that of lemons. It effervesces with alkalis, and unites with them as other acids do, but it does not affect the vegetable tinctures ſo much.

Origin.——It is extracted from the vegetable ſalt called tartar, which contains this acid combined with an alkali, and which is found in the juices of many of the vegetables, particularly in that of the grape. The nature of this compound ſalt, which was not thoroughly underſtood, until it was inveſtigated by Mr. Scheele of Sweden, ſhall be explained when we treat of the compound ſalts.

Beſide the tartar commonly known and uſed in medicine, there are other kinds, a little different from it, in the juices of other vegetables; as ſorrel, tamarinds, &c. The difference of them from common tartar depends upon their acid, which is a little different, though it agrees in its nature upon the whole; all of them being totally deſtructible by a burning heat, and with the ſame phenomena as thoſe that accompany the deſtruction of the tartarous acid of wine.

For a great variety of vegetable acids, many of them newly diſcovered, ſee Fourcroy's Preliminary Diſcourſe to the ſecond edition of his Elements.

SPECIES VI.—SEDATIVE SALT.

The ſedative ſalt, the next in order, was ſo called from ſome virtues in medicine, which it was ſuppoſed to have when it was firſt diſcovered. Its powers as an acid are ſtill weaker than thoſe of the vegetable acids. We can hardly perceive any acidity in it by the taſte, and it has very little effect on the vegetable colours. But it effervesces with alkalis, and unites with them in the ſame manner as the acids, and it can be combined with ſome of the earths which have attraction for acids in general. It is got from a compound ſalt called borax, and the nature of the operation by which it is extracted from the borax, is ſuch, that the ſedative ſalt is obtained in a cryſtallized ſtate; the cryſtals which it forms being moſtly flat and thin like the ſcales of fiſh, and feeling ſlippery between the fingers. When we make experiments upon it in this ſtate, we find two things very remarkable in it:

The firſt is the manner in which it is affected by heat applied to it. When expoſed to heat in the open air, it generally emits white fumes, and ſeems to melt; but this is only a watery fuſion, and it ſoon becomes dry and ſpongy. If it be heated in a retort, it firſt undergoes the watery fuſion; then water is diſtilled from it; ſome of the ſalt ſublimes, and the reſt remains perfectly fixed. Were we to form a concluſion too haſtily from this experiment, we might imagine that this ſalt is not homogeneous; but that part is fixed and a part volatile. But we ſhall be undeceived if we purſue the experiment. If we collect the ſublimed matter and examine it, we find it to be preciſely the ſame as the ſedative ſalt was at firſt, only formed into ſmaller, thinner, and lighter ſcaly cryſtals. When theſe are expoſed to heat in the ſame manner, they ſhew preciſely the ſame phenomena which have been mentioned above. On the other hand, the fixed part which remains in the retort is hard and tranſparent like glaſs or horn. But it

diſſolves in hot water, and cryſtallizes on cooling, and undergoes the ſame changes as that which was ſublimed.

Theſe experiments, therefore, ſhew that there is no difference between one part of this ſalt and another, but that the manner in which a part of it ſublimes is this: The cryſtals of it, like thoſe of many other ſalts, contain a good deal of water. When heat is applied to theſe cryſtals, watery fuſion takes place, and the water is gradually driven off in the form of vapour. While the quantity of the water which adheres to the ſalt is thus diminiſhed, the remainder is retained the more ſtrongly; the greater proportion of ſaline matter with which it is combined, acting upon it with a greater power of attraction; and when the heat increaſes to ſuch a degree as to force it to riſe, a part of the ſalt riſes with it. They cohere too ſtrongly to be completely ſeparated by heat. At laſt, however, ſo much of the water is ſeparated, that none, or ſcarcely any, remains with the ſalt in the bottom of the retort. The ſaline matter then ſhews its natural relation to heat. It is fixed in the fire, and melts into a tranſparent viſcid fluid like glaſs; but this glaſs-like matter can all be made to ſublime by repeated additions of water.

In this manner is the ſedative ſalt affected by heat. The other remarkable particular obſervable in it, is the effect it produces on ſpirit of wine. Sedative ſalt diſſolves very readily in highly rectified ſpirit of wine, eſpecially if previouſly deprived of all water; and, in this ſtate it may be taken up by the wick of a lamp, or any bibulous ſubſtance. The beſt way of obſerving its effect on the flame, is to dip a bit of paper into the ſolution, and hold it upright while burning. The flame is of ſhort duration, but very large, and of a beautiful green colour, eſpecially at the top.

Although this ſalt ſhews ſtrong attraction for the water which it contains in its cryſtals, it has but little diſpoſition to unite with more water, or to diſſolve in it. Cold water diſſolves very little of it. Hot

water has much more effect, but when the ſolution cools, the greater part cryſtallizes.

The origin and natural hiſtory of the ſedative ſalt was, until of late, very obſcure. All that we knew of it was, that it is a part of a compound ſalt called Borax, imported from Boutan and Thibet, in the Eaſt Indies. But when inquiry was made about the origin of the compound ſalt, we could obtain no diſtinct accounts of it; reports only bearing that it was got from certain mineral waters. This, however, was not ſatisfactory, becauſe nothing like it was known in Europe or America.

At laſt, however, two or three years ago, ſome hot mineral waters were diſcovered by Hoefer in Italy, in the neighbourhood of Siena, and alſo in the Duke of Tuſcany's dominions, which, beſide other ingredients, contain ſedative ſalt. (*Extrait des Obſervations ſur les Lagonis au pais de Siena et celui de Volterra en Toſcan, par Paul Muſcagne. Obſervations de Phyſ.* 1780.) The deſcription of the neighbourhood of the ponds where the ſedative ſalt is found, and the account of the changes which are inceſſantly going on there, in which the firmeſt flinty rocks are continually mouldering away, and forming new compounds, is extremely curious, and will lead the philoſophical chemiſt to very important reflections. In theſe places, the ſedative ſalt is ſometimes found in conſiderable maſſes, generally adhering to a ſchiſt or ſlate, in cryſtals conſiderably differing in form from what I have been deſcribing. It is alſo frequently found combined with other ſubſtances, ſuch as lime, clay, volatile alkali; but he has not found it in that ſtate in which it is gotten in India.—See alſo *Experiences ſur les ſels ſedatifs, nitreux, marin, et aceteux, par M. Cadet, Ac. Roy.* 1780.

It is alſo found in a very hard ſtone at Luneburg in Germany, and may probably be got in others.

As I have already obſerved, there are ſeveral other ſubſtances having the chemical properties of acids. Amber furniſhes one; ants yield another. Several plants contain acid juices; ſuch as ſorrel, rhaponti-

cum, and the whole family of the rumices or docks. Moſt of the pulpy and fleſhy fruits of trees and ſhrubs yield it in abundance. Theſe are all of a very complex nature; and are eminently diſtinguiſhed from the mineral acids, in their being deſtructible by fire, in ſuch a way that they cannot be recovered. You are not yet prepared for underſtanding their diſtinguiſhing properties. Indeed theſe are not yet very fully inveſtigated, having but lately attracted much attention.

Beſides theſe, there have been diſcovered two other acids, which are not deſtructible by fire, namely the acid of phoſphorus, and the acid of fluor or ſpar. Theſe have very diſtinct and very remarkable properties, and will be duly noticed as we proceed.

GENUS III.

OF THE COMPOUND OR NEUTRAL SALTS.

HAVING now given ſome account of two of the moſt active claſſes of chemical bodies, the alkalis and the acids, and having taken ſome notice of their diſpoſition to unite and combine with each other, I ſhall take this firſt opportunity of making you familiar with the moſt important part of chemical ſcience and art,—the compoſition, and decompoſition of bodies. The relation of the alkalis and acids afford the beſt example of this. The phenomena are ſimple and preciſe, and therefore are the moſt proper ſpecimens. Beſides, the union and ſeparation of acids and alkalis occur in almoſt every other chemical inveſtigation.

I have conſidered three alkaline ſalts, and ſix acids; and I am now to explain how theſe nine ſalts, or ſaline ſubſtances, unite with one another to form the compound ſalts, which have been a long time known and in uſe.

A perſon who knew nothing more concerning the compound ſalts which we are now to deſcribe, but that they are formed by different combinations of the nine ſimpler ſalts which you have ſeen, might naturally ſuppoſe their number to be very great; for, were the ſimpler ſalts capable of being combined in a great variety of ways, in reſpect of number and proportions of the ingredients, the different

compounds producible from them would amount to a very great number indeed. But when you call to mind the nature of chemical union in general, and the account which I gave of the different ſimple ſalts, with reſpect to their power of combination, that is, the limited manner in which alone they are diſpoſed to unite, you will perceive that we can produce from them but a very moderate number of compound ſalts.

For, in the firſt place, it appears by mixing them variouſly together, that no acid has any attraction for any other acid, nor any alkali for other alkalis. Acids may be mixed intimately with acids, and alkalis with alkalis, becauſe they can be applied to one another in the form of watery fluids; but thus mixed, they do not ſhew any chemical attraction for one another. The volatility, and other properties of the volatile acids, are not in the leaſt diminiſhed by mixture with the fixed acids, nor the volatility of the volatile alkali, by the fixed alkalis. The only manner in which we can obtain compound ſalts, is by mixing the acids with the alkalis. Theſe, upon all occaſions, ſhew a chemical attraction for one another, and are ſtrongly diſpoſed to unite; and their union, when we employ them in their ordinary ſtate, is always accompanied with that violent effervescence which you have ſeen.

But what ſtill farther limits the number of ſalts producible in this way is, that almoſt in every caſe the acid and alkali are capable of uniting firmly together, in one certain proportion only. What the nature of this proportion is, we cannot pretend to ſay; whether it conſiſts of an equality in the number of the atoms of the two ſalts, or whether there muſt be two acid particles or atoms to every one of the alkali, or two of the alkali to one of the acid, or what other proportion, different from either of theſe, may be neceſſary to conſtitute the compound ſalt, cannot be determined. But, in order to explain the formation of theſe ſalts more diſtinctly, I ſhall ſuppoſe that the proportion in which the acid and alkali are diſpoſed to unite the

moſt ſtrongly, is that of equality. Let us then take an acid and an alkali diſſolved ſeparately in water, and add a ſmall quantity of the acid liquor to the alkaline one; every atom of the acid that has been added is united with a reſpective atom of the alkali, ſo that a certain quantity of the alkali is changed into a compound ſalt, which, if there be water enough to diſſolve it, is equally diſtributed through the liquor, and blended with the remaining alkali. But this remaining alkali ſtill retains all its properties, and is preciſely the ſame as before. If we go on to add more acid by degrees, we ſhall ſucceſſively change more of the alkali into a compound ſalt, until ſuch a number of acid atoms have been added as are ſufficient to change the whole of it. Then the mixture is complete. If we add more of the acid, it will ſuffer no change; it will be uniformly diffuſed through the liquor, but will remain diſengaged, and retain all its properties; or, if a ſmall quantity of it ſometimes attaches itſelf to the particles of the compound ſalt, its adheſion is looſe and imperfect, and eaſily overcome. This is not only the caſe with regard to the ſame acid which enters into the compoſition of the compound ſalt, but even although we add any other acid, it will either produce no effect whatever, or, if it does unite itſelf to the alkali, it is ſure to ſeparate the firſt acid, ſo as to produce a different compound ſalt, but one of a ſimilar nature to the former, in ſo far as there is a limit to the proportion of acid and alkali of which it is compoſed. What has now been ſaid with reſpect to the mixture of different acids with the ſame alkali, muſt be underſtood alſo of different alkalis mixed with the ſame acid. As the ſame alkaline atoms are incapable of being ſtrongly united by attraction with two or more different ſpecies of acids at the ſame time, ſo the ſame acid particles are incapable of being united at the ſame time with more than one ſpecies of alkali.

I have been thus particular in explaining the manner in which acids and alkalis unite to form compound ſalts, for this reaſon, that

the ſimpler ſalts, and eſpecially the acids, are among the moſt active of the objects of chemiſtry. They diſſolve a variety of earths, metals, and other bodies, and unite with theſe in a manner very much ſimilar to their manner of uniting with alkalis; upon which account it was proper to explain this once for all at full length. On this ſubject I recommend to your ſerious peruſal Mr. Kirwan's diſſertation on the attractive forces of the mineral acids, publiſhed in the Philoſophical Tranſactions, and alſo in a ſeparate volume. You will there meet with many important obſervations on the proportions of the ingredients, and the variations to which this is ſubject, and many other inſtructive particulars. Mr. Berthollet has alſo conſidered this ſubject with peculiar care and great judgment.

The compound ſalts, however, are not always produced by joining together artificially the pure acid and alkali which they contain. Some of them are found ready formed by nature; others are produced accidentally, in chemical proceſſes, in which their conſtituent ſalts are mixed together, although their union is not the principal object of the operation. But there are ſome which we never obtain but by mixing, on purpoſe, the acid and the alkali neceſſary to their compoſition.

When we produce a compound ſalt in this way, and gradually add the acid and alkali to one another, in order to hit the due proportion, this is called ſaturating theſe ſalts with one another; and when the due proportion is found, we ſay they are mutually ſaturated, or that we have hit the point of ſaturation. This point is judged of commonly by the gradual diminution and ceſſation of effervеſcence. But there are other marks which are more certain and exact ſigns of ſaturation. If we truſt to the ceſſation of effervеſcence alone, we may commit miſtakes, eſpecially if we do not take care to agitate the mixture very frequently and very briſkly, while we are adding the one ſalt to the other.

Thus if we add an acid to an alkali by degrees, till the effervescence ceases, we shall afterwards find that we have added too much acid, for the liquor will now effervesce by the addition of alkali. The same thing will happen if we begin by adding alkali to an acid. It was proper to take notice of this circumstance, because it is a general fact in all effervescing mixtures. Nothing will secure us against the danger of exceeding in one of the ingredients, but frequent and brisk agitation of the mixture, especially as we approach to the term of saturation.

But better proofs of exact saturation may be had by examining the state and qualities of the mixture; for the compound salts thus produced have very different qualities from either acid or alkali. The acid in a separate state has a strong sour taste; the alkali has a different one peculiar to the alkaline salts. The acid gives a red colour to the blue or purple tinctures of vegetable flowers; the alkali gives a green. But when they are properly united, the compound salt which they produced has not the taste of either acid or alkali, but one that is much milder, and of a quite different kind. Moreover, it occasions no change of colour in the vegetable infusions.

By these marks, therefore, we can bring the mixture to more exact saturation, particularly by vegetable colours. And the best manner of using the vegetable colours is to have some paper or fine linen stained with them: little bits of which being dipped into the mixture, will immediately shew, by the change of colour, whether there is any superfluous acid or alkali in the mixture; and it is best to have different kinds of these for different purposes. Thus the juice of the flowers of the March violet is the most sensible to alkalis; and the infusion of the dying drug, known by the name of Litmus or Lacmus, is the most sensible to acids, though little affected by alkalis. There is also a coloured infusion prepared from red cabbage, which is recommended by Mr. Watt, in a paper in the Philosophical Transactions, as a most delicate trial of the presence of acid; and the purple skin of the com-

mon radiſh, when ſcraped off with a knife, gives a juice with which paper may be ſtained for this purpoſe, ſo as to afford a very nice trial of acid or alkali in liquors.

In ſome caſes, however, it is not neceſſary to be extremely nice, becauſe the nature of the particular ſalt produced is ſuch, that though the acid or alkali be redundant, the compound ſalt is eaſily ſeparable from this redundant acid or alkali, by cryſtallization or otherwiſe. Thus, when we compoſe a ſalt of an acid and the volatile alkali, we chooſe rather to add a little too much than too little of the volatile alkali, becauſe this alkali is ſo volatile, that when the water is afterwards evaporated, we are ſure that all that is ſuperfluous will fly away; and we ſhall ſee afterwards that cryſtallization likewiſe, in ſome caſes, will purify a compound ſalt from acid or alkali.

The other particulars by which compound ſalts differ from acids and alkalis, are, 1*ſt*, A weaker attraction for water, ſo that in general they are eaſily ſeparable, and dried or cryſtallized; 2*d*, Much leſs acrimony than either of them. Moſt of them do not act at all, and the reſt but weakly, on metallic and earthy bodies, and inflammable ſubſtances. From this variety of effects, the acids and alkalis have been conſidered as contrary or oppoſite in their nature, and as qualifying and counteracting one another in the compound ſalt. Hence have ariſen the names of *neutral ſalts* and *ſales medii*; though, when they are conſidered more attentively, there ſeems to be but little foundation for this ſuppoſition of their contrariety. The whole is perfectly agreeable to that general obſervation, given formerly, with regard to mixture, that when two bodies are united by a ſtrong attraction, their activity or acrimony, and their attraction for other bodies, is very much diminiſhed, or entirely diſappears. The epithet, neutral, is applied to the compound ſalts with conſiderable propriety, becauſe the compound has neither the properties of the one nor of the other ingredient. Middle ſalts is a leſs proper name.

From the view which I have given of the nature of compound

ſalts, you muſt now perceive very plainly, that all the variety producible from the nine ſimpler ſalts already deſcribed, amount only to eighteen; for each of the alkalis is only capable of being combined with any of the ſix acids, ſo that with three alkalis and ſix acids we can produce only eighteen compound ſalts. Even of theſe eighteen, ſome have not been examined, or applied to any uſe, and therefore are not commonly prepared or treated of in chemical books.

By thus conſidering the poſſible number, you perceive that they can be reduced to the form of a table, in ſuch a manner that the place of each ſhall point out the acid and alkali of which it is compoſed:

TABLE OF NEUTRAL SALTS.

	Vitriolic Acid.	Nitrous Acid.	Muriatic Acid.	Acetous Acid.	Tartarous Acid.	Sedative Salt.
Vegetable Alkali.	Vitriolated Tartar.	Nitre.	Sal Digeſtivus.	Sal Diureticus.	Tart. Solub. and Tartar.	
Foſſil Alkali.	Glauber's Salt.	Cubical Nitre.	Common Salt.		Sal Rupellenſis. Rochelle Salt.	Borax.
Volatile Alkali.	Vitriolic Ammoniac.	Nitrous Ammoniac.	Sal Ammoniac.	Spiritus Mindereri.		

The neutral ſalts may be decompoſed, and the ſimpler ſalts of which they conſiſt ſeparated from one another, in ſome caſes, by heat alone, but in moſt caſes by elective attraction, or the addition of ſome third ſubſtance, which attracts one ingredient of the ſalt more ſtrongly than the ingredients attract one another, and, therefore, unites with that ingredient, and ſeparates the other, or throws it looſe. In moſt caſes, an addition of only one of the ſimpler ſalts is required; for it has been diſcovered by experiments, in mixing neutral ſalts with ſeveral kinds of the ſimpler ſalts, that the different acids have different degrees of attraction for the alkalis in general, while the alkalis, on the other hand, have different attractions for the acids in general. If, therefore,

we wiſh to have the acid of a neutral ſalt pure, it is in our power, with regard to moſt of the ſalts, to obtain it by the addition of another acid and heat. On the other hand, we can likewiſe obtain the pure alkali of a compound, by the addition of another alkali.

It is very adviſable for a ſtudent of chemiſtry to have this table very familiar to his mind. Much more may be learned from it, than merely that ſuch a compound ſalt is made by the mixture of ſuch an acid and ſuch an alkali: For the table, when properly conſtructed, may be made a picture, as it were, of a great train of chemical relations and operations, and tell us what will reſult from the mixture of a compound ſalt, with other acids or alkalis than thoſe of which it conſiſts. For it fortunately happens, that the elective attractions of any acid for the different alkalis proceed in the ſame order. The ſame thing obtains in the elective attractions of an alkali for the different acids. Therefore the columns of the table may be arranged according to the elective attractions of an alkali for the different acids; and the horizontal rows may be arranged by the attractions of an acid for the different alkalis: Or the columns may be arranged by the attractions of the acids, and the rows by thoſe of the alkalis. I have choſen the firſt method. The firſt column and the firſt row contain the ſtrongeſt, and the laſt of both contain the weakeſt attractions. In the ſpace allotted to each compound ſalt, I have inſerted the name by which it has been moſt generally mentioned by the ſyſtematic writers, till of late. They are mentioned by different authors by a great variety of names, according to the particular views of the writer, whether medical, philoſophical, or otherwiſe. It is proper, therefore, to ſubjoin to this table a complete liſt of ſynonima, in the manner of the natural hiſtorian. The new ſyſtematic names, and a different arrangement of the compound ſalts, will be laid before you after this, when we ſhall have underſtood the principles which have given riſe to them.

I ſhall now deſcribe the compound ſalts in the order in which they ſtand in the table.

SPECIES I.—VITRIOLATED TARTAR.

Firſt, therefore, we conſider vitriolated tartar, ſo called becauſe compoſed of vitriolic acid and ſalt of tartar, which laſt is now known to be the pureſt form of the vegetable alkali. Hence it is named in the Edinburgh Pharmacopœia, *Alkali Fixum Vegetabile Vitriolatum*, a denomination rather long and cumberſome. It is now called *Sulphat of Potaſh*.

Its taſte is a little pungent and bitteriſh, and half an ounce of it purges. Smaller doſes are given as laxatives and to promote the other ſecretions, and it is ſometimes uſeful in ſcrophulous caſes. As it contains a fixed alkali, and the leaſt volatile of the foſſil acids, and as theſe two ſalts are united in it by the ſtrongeſt elective attraction, they cannot be ſeparated from one another by heat alone; nor can any ſimple ſalt be employed in this caſe to produce their ſeparation. It was, therefore, ſome time ago, reckoned a very difficult problem to ſeparate the vitriolic acid from the vegetable fixed alkali; and Dr. Stahl raiſed much emulation among the chemiſts, when he boaſted of his knowing a method ſo eaſy, that he could perform it in the hollow of his hand. This occaſioned many attempts, and produced a diſcovery of ſeveral methods for decompounding this ſalt, which may be performed more or leſs perfectly in the hollow of the hand. Moſt of theſe depend on caſes of double elective attraction, which it will be more proper to explain hereafter than now. The manner of doing it which Stahl ſeems to have meant, depends upon a preparatory operation, by which the acid is converted into ſulphur. I have already mentioned this preparatory experiment, when treating of the vitriolic acid. Stahl's mixture of vitriolic acid and alkali (to be expoſed along with charcoal to a red heat) was this very ſalt. Sulphur was produced, and, therefore, ſome of the alkali was left ſeparated. The ſul-

phur unites with this, compoſing *hepar ſulphuris*. The union is ſo weak, that vinegar will ſeparate the ſulphur, and form a compound ſalt with the alkali. Then are the two ingredients completely ſeparated. They may even be had pure; for we can burn the ſulphur, and thus obtain the acid. And, the compound ſalt being expoſed to a red heat, the vinegar is deſtroyed, and thus we obtain the alkali.

This experiment has been very inſtructive. Being attempted in a great variety of ways, it brought many things to light, and was particularly uſeful, by explaining the frequent appearance of ſulphur in mixtures of ſubſtances which did not contain it. They were all caſes of vitriolic acid urged by red heat in contact with inflammable bodies.

This compound ſalt has not a ſtrong attraction for water. It diſſolves but ſlowly, and in ſmall proportion, in cold water; and even of hot water a conſiderable quantity is required to its diſſolution. It readily cryſtallizes, and its cryſtals are of the decrepitating kind, and contain but little water in their compoſition. According to Bergmann they contain,

Alkali	52	in 100 parts by weight.
Acid	40	
Water	8	

The form affected by the cryſtals is a hexangular priſm, terminated by a pyramid. But this is diſtinguiſhed with difficulty, by reaſon of their grouping together. Croſſing at right angles is a pretty diſtinguiſhing mark of this ſalt *. But although they ſhew ſo little attraction for water, it will carry off a good deal of the ſalt with it, if we evaporate too haſtily.

Origin.——It is not found as a foſſil production. There is ſome of

* The cryſtals of vitriolated tartar form a double priſmatic ſpectrum by refraction, in the ſame manner as ſeveral kinds of rock cryſtal.

EDITOR.

it in the aſhes of vegetables mixed with the alkali, and it is probably formed in their juices by the powers of vegetation. It is commonly prepared for the purpoſes of medicine by art. We ſhall ſoon ſee an example of this in a proceſs in which the production of it is not the principal object. In order to obtain it in its faireſt form, with the largeſt and moſt regular cryſtals, it is adviſable to have the acid predominant in the mixture. Although it be conſiderably acid, the cryſtals will be perfectly neutral. Some chemiſts, however, chooſe to call ſuch a compound a ſuper-ſulphurated ſalt.

SPECIES II.—GLAUBER'S SALT.

Glauber's ſalt is called *Soda Vitriolata*, in the Edinburgh Pharmacopœia. Its new name is *Sulphat of Soda*.

This ſalt is much more eaſily melted in the fire than vitriolated tartar, and it is alſo more ſoluble in water. Its cryſtals are in the form of long hexagonal priſms, terminated by pyramids. The priſms have two broad and four narrow faces. They are very watery, and ſubject to watery fuſion, and fatiſcence. Hence for a purgative doſe of it an ounce is required. 100 parts of it contain 15 of alkali, 27 of acid, and 58 of water. It cannot be decompounded by heat alone; but, like vitriolated tartar, by ſulphurification and vegetable acid; and by ſome other proceſſes which depend on double exchanges. We can alſo decompoſe it, though not conveniently, by the action of the vegetable alkali. *Oleum Tartari per Deliquium*, which is the mildeſt form of the vegetable alkali, muſt be digeſted on Glauber's ſalt for a long while, and we ſhall then find a vitriolated tartar, and a fixed foſſil alkali in the mixture. The attraction of the vegetable alkali for the acid does exceed that of the foſſil alkali, as may be gathered from many experiments; but it is ſo little, that it was long doubted. Even the preſent decompoſition of vitriolated tartar does not prove it, becauſe, as we ſhall ſoon ſee, it is aſſiſted by a double exchange.

Origin.——This ſalt is no where found, that we know of, as a natural production. Many, indeed, ſay that it is contained in the waters of the ſea, and in thoſe of purging mineral ſprings. But this is a miſtake, and the ſubſtance which we take for it is the bitter purging ſalt, which was long miſtaken for Glauber's ſalt, even by chemiſts of great eminence. Mr. Model of Peterſburgh relates a number of experiments made by him on a ſalt found in many waters in that neighbourhood, and deſcribes the ſalt ſo exactly, that we now know beyond any doubt that what he calls the Sal Mirabile is nothing but Epſom ſalt.

All of it that is uſed in medicine is artificially made, by decompoſing common ſalt with vitriolic acid; and the manufacture of it forms a conſiderable buſineſs in the hands of trading chemiſts. Some of it is formed from kelp. The following proceſs for making it is publiſhed by the Society of Arts:—Put a pound of kelp into an Engliſh gallon of boiling water, ſo as to extract the ſalts. Decant, &c. and add as much vitriolic acid as is neceſſary to ſaturation. Two ounces are commonly enough. Then filtrate, evaporate, and cryſtallize, &c. and you will have about half a pound of Glauber's ſalt.

SPECIES III.—NITRE OR SALTPETRE.

This ſalt is well known, and is very uſeful and neceſſary in many arts. It has occaſioned a total change in the art of war, for the uſe of it in that art was unknown to the ancients.

I have already obſerved, that the ſalt mentioned in ſome parts of the Bible by the name of nitre, was an alkaline ſalt. This appears from the contraſt or incongruity remarked between it and vinegar, and alſo from its being employed as a detergent or cleanſer. In moſt of the paſſages alſo of Pliny, where he ſpeaks of nitre, he plainly means an alkaline ſalt. It is very clear, however, that our ſaltpetre was known by that name, and alſo by the name of nitre, in the ear-

lieſt ages of ſcientific chemiſtry. It is frequently mentioned, and with characters which give us no room to doubt of its identity.

In purſuance of my general method of examination, I ſhall firſt conſider the moſt ſimple relations of nitre to heat, and its mixture with water, and then proceed to relations of the ſame kind, but more complicated in their circumſtances. I ſhall employ a good deal of time in this examination, both becauſe the chemical properties of nitre, and the phenomena which it exhibits, are the moſt remarkable that occur in the chemical hiſtory of nature, and alſo becauſe theſe properties and phenomena afford, in the eaſieſt manner, the ſteps of inveſtigation which lead us through almoſt the whole philoſophy of our courſe. You will find the greateſt part of our important doctrines to be little more than the gradual detection of the properties of nitre and its ingredients. I could begin by ſtating all thoſe properties at once, in their full influence, and from thence deduce the chemical doctrines ſynthetically, as Newton explains the colours of natural bodies from the properties of light, or the motions of the planets by univerſal gravitation. But, to give you the conviction that ſuch are in fact the properties of nitre, would require the narration of facts concerning many ſubſtances with whoſe properties you are altogether unacquainted. I am certain that your conviction will be much more forcible, and your knowledge more palpable, and, as it were, at your finger ends, by allowing thoſe properties of nitre to open upon you by degrees, as we come to conſider thoſe other ſubſtances, whoſe appearances, as affected by nitre or its ingredients, are the very arguments by which the properties of thoſe ingredients are aſcertained. The great doctrines of modern chemiſtry will thus be the reſult and fruits of your own induction and analyſis. Add to this, that you will thus have formed the habit of philoſophical inveſtigation by experiment, and will perceive its immenſe ſuperiority over all ſynthetical theory, however ſpecious and extenſive in its application. I may add, that were I to treat theſe ſubjects in the pure ſyn-

thetical method, which is thought attainable, and which ſeems, at preſent, to be moſt acceptable to the chemiſts, (eſpecially on the Continent), I ſhould leave you ignorant of the ingenious labours of the moſt eminent chemiſts of Europe, and unable to read and underſtand their writings.

When nitre is expoſed to heat in an open veſſel, ſuch as a crucible, we find very little water in its cryſtals. It neither ſhews watery fuſion, efflorescence, nor decrepitation. Its water eſcapes inſenſibly. If the heat be very haſtily applied, ſome nitre ſublimes, very little changed; but a ſlow and gradual increaſe of heat melts the nitre quietly, before it be ſo hot as to ſhine in the dark. It looks like melted white bees wax. If the heat be increaſed a little, the fluid begins to ſimmer gently, by the eſcape of very ſmall bubbles of elaſtic matter. If this be long continued, the diſagreeable ſmell of nitrous acid is perceived, and fumes are emitted of a deep blood-red, perfectly reſembling thoſe already deſcribed, when I was conſidering the nitrous acid. By this proceſs, the nitre gradually waſtes, and what remains frequently diſſolves the glaſs or earthen veſſel in which the nitre was contained. The reſiduum, when examined, is found to be ſtrongly, and ſometimes entirely, alkaline, and quickly deliquiates in the air.

From this it would appear that nitre is decompoſed by heat, the acid being driven off in theſe red vapours. We are naturally led, therefore, to try the effect of diſtillation in cloſe veſſels; but we ſhall find it extremely difficult to condenſe the vapours; and the quantity of acid obtained in this way is frequently almoſt nothing; in all caſes, it is fuming and deep coloured, and the veſſels are filled with blood-red vapours. They are incoercible, and would burſt the veſſels; yet the acid is gone from a part of the nitre at leaſt. Since we cannot collect it in the receiver, we muſt conclude that it is ſomehow deſtroyed or decompoſed, or converted into theſe permanently elaſtic fluids. Accordingly, Dr. Hales obtained from a cubic inch of nitre in this way, (that is, by diſtilling it in a gun-barrel, and collecting the vapour in a

jar ſtanding inverted in water) 180 cubic inches of air. Dr. Hales explains, by means of this air, the exploſion of gunpowder and other nitrous compoſitions. In the preceding century, ſomething like this had been done by Dr. Mayhow of Oxford, in 1674. He collected the nitro-aërial ſpirit, as he called it, in bladders tied on the ſpout of the retort. (See *Jo. Mayhow Opera Medico-phyſica*, publiſhed at the Hague, 1681; alſo his *Diſſertatio de Salnitro et de Spiritu Nitri Aëreo*). This author writes in an obſcure myſterious manner, as was too much practiſed in thoſe days; but he has made many experiments, moſt ingeniouſly contrived, and has deduced from them concluſions which are nearly the ſame with the doctrines which have been more clearly eſtabliſhed of late years. In particular, he conſiders this nitro-aërial ſpirit as the cauſe of combuſtion, the ſupport of animal life, and the ſource of animal heat. Since that time, and even before, there has been much vague ſpeculation and diſcourſe about a vivifying nitrous ſpirit in the air. Muſchenbroeck and others aſcribe to it the ſpicular form of ſnow and hoar-froſt; but all this was mere talk, without any diſtinct thoughts on the ſubject. *(See Note* 31. *at the end of the Volume.)* I ſhall very ſoon give a more particular account of this elaſtic matter.

The relation of ſaltpetre to water offers nothing very remarkable. It diſſolves readily in ſix times its weight of cold water. Boiling water, however, diſſolves an equal weight. In both caſes, a conſiderable abſorption of heat takes place. It is eaſily ſeparated from water by cryſtallization, and the abſorbed heat again emerges. The cryſtals are ſlender hexagonal priſms, of equal ſides, terminated by irregular pyramids, having one face much broader than the reſt. They are thus diſtinguiſhed from Glauber's ſalt, whoſe priſms have very unequal ſides. Nitrous cryſtals contain very little water, but ſo ſtrongly united as not to be ſeparable without a red heat.

The moſt remarkable properties of nitre appear when it is treated in great heats in contact with inflammable ſubſtances. When char-

coal, or ſulphur, or any inflammable which is not ſo volatile as to riſe before it get a red heat, be put into melted nitre, the nitre, which was quietly fluid, is immediately thrown into violent agitation, and burſts out in exploſions of elaſtic matter and dazzling flame. This continues, with a whizzing noiſe, as long as inflammable matter remains; and, if the quantity be conſiderable, and intimately mixed, the whole explodes in a moment, with almoſt irreſiſtible force, a bright flaſh, and loud noiſe. If a lump of charcoal be laid on nitre, juſt hot enough to act on it, the deflagration takes place only in the touching parts, and it continues whizzing, and throwing out ſtreams of fire, till the coal is expended. Great heat is produced, and the nitre will all become of a bright red. Even although the nitre be not ſo hot as to ſet fire to the charcoal, if only a bit of the coal be red hot, it will there be acted on by the nitre, and the heat produced will ſuffice for making the adjoining parts act on each other; and the detonation will continue till the charcoal be expended, and the whole nitre be melted and become red hot.

This effect of nitre on combuſtible ſubſtances is called DEFLAGRATION, or DETONATION. No wonder that chemiſts have attempted to explain the grand operations of thunder and lightning, by ſuppoſing mixtures of this ſalt, or of its nitro-aërial ſpirit, with inflammable ſubſtances in the air. Indeed it was thoſe phenomena of nature, in all probability, which gave riſe to the notion that ſuch a ſpirit was diffuſed through the atmoſphere. But thunder and lightning are now well known to depend on very different principles.

By repeated additions of charcoal, the deflagration, and the fuſibility of the nitre, are diminiſhed, and the deflagration at laſt ceaſes entirely. When the ſalt remaining is examined, it is found to be pure vegetable alkali, in quantity juſt equal to what we know the nitre to contain.

This fact gives an explanation of the whole phenomenon. It is produced by the violent action of the nitrous acid and inflam-

mable matter. This is already known to you. The acid quits the alkali to act on the combuſtible matter, and the action is ſuch, that both are converted into elaſtic aërial fluids, ſo completely, that no part of the acid can be recovered in its former ſtate: Indeed it is decompounded and deſtroyed. Of this we obtain clear proof, when the operation is performed in a ſtoppered earthen retort, with a receiver for condenſing the volatile matters. We find no acid in thoſe vapours, but, in the place of it, a large quantity of air, of a complex kind. It will not ſupport flame nor animal life; and it was therefore imagined to be of that kind that is met with in coal-mines, and is called choak-damp, and in veſſels where charcoal has burnt till it is extinguiſhed for want of air. But when we had learned an exact teſt of ſuch air, and the method of abſtracting it from any mixture, it was then found that the air produced by deflagrating charcoal with nitre did contain a conſiderable portion of it, but that another portion was of that kind that is left unabſorbed by *hepar ſulphuris* expoſed to atmoſpheric air, and is called *faul* air by Scheele, and *phlogiſticated* air by Prieſtley; and, beſides theſe two, it uſually contains ſome common air. This was extricated from the materials, and it is evident, by the alkaline reſiduum, that the acid alone contributed to the production. We ſhall ſoon underſtand the change thus produced on the acid; and I only obſerve at preſent, that a red heat is neceſſary for occaſioning this action. The acid, having a ſtrong attraction for the alkali, is thus weakened in its attraction for inflammables; but the union with the fixed baſis enables it to ſtand the red heat which is neceſſary. In a ſeparate ſtate, we have ſeen it act moſt violently, and even excite them to inflammation; but we are not therefore to ſuppoſe that the heat produces this deflagration by firſt decompoſing the acid, and changing it to elaſtic gas, which we have ſeen that it really does. Yet, no doubt, this favours the detonation: For when nitre is melted, and red hot, and its acid eſcaping in red fumes, if a bit of charcoal be held cloſe above it, but not touching, it

will preſently take fire, and burn with rapidity and dazzling brightneſs; an evident proof that it is the elaſtic matter of the nitre which cauſes the charcoal to burn, and that the acid muſt be in that form, but not yet decompoſed, when it produces the effect.

When nitre is deflagrated with ſulphur, the conſequences are conſiderably different. The deflagration is equally violent with the other, and the flame ſomewhat more brilliant. The acid is deſtroyed in the ſame manner. But the elaſtic matter produced is different in ſome reſpects. The moſt of it is the faul air of Dr. Scheele; a circumſtance deſerving particular notice. We have an evident ſmell of *hepar ſulphuris*, as we might expect, from the combination of the ſulphur with the alkali left behind in the deflagration. The reſiduum of the proceſs is not alkali, but vitriolated tartar. Before its chemical nature was thoroughly known, this reſiduum had gotten a high name as a medicine, uſeful in a variety of diſorders. It was, therefore, called *Sal Polychreſtus*, *Arcanum Duplicatum*, *Sal Prunellæ*, and other fanciful names.

The *rationale* of this proceſs is obvious. There is a double exchange, and the acid of the vitriolated tartar proceeds from the ſulphur, which unites with part of the air diſengaged from the nitrous acid.

This faculty of deflagration is a moſt valuable property of nitre to the philoſophical chemiſt. It enables him to diſcover inflammable matter where it could not otherwiſe be ſuppoſed to exiſt. It is a property no leſs important to the world on other accounts. Nitre becomes the baſis of many compoſitions for producing violent exploſions, or a dazzling light, which may be ſeen at a great diſtance.

Gunpowder is the moſt generally known of all theſe exploding compoſitions. Its many uſes, beſides the art of war, are well known. It is formed of nitre, charcoal, and ſulphur, ſo intimately mixed, that a ſpark fires an atom or two, and this fires the reſt in a moment.

The beſt proportion for ſtrong gunpowder is about ſeven parts of pure nitre, one of charcoal, and one of ſulphur, all by weight. Others recommend eight of nitre, two of charcoal, and one of ſulphur. The uſual impurity of ſaltpetre is common ſalt, diſengaged alkali, and Epſom ſalt. Theſe, by attracting moiſture, weaken the powder exceedingly, and make it apt to cake in the barrels. But ſaltpetre, ſufficiently pure, is an expenſive article, and is ſtinted as much as poſſible in the manufacture. Common gunpowder has a much ſmaller proportion, even ſo little as three or four parts to the above quantity of the other ingredients.

The charcoal and ſulphur are ground ſeparately in mills, and levigated to moſt impalpable fineneſs. On this depends much of the ſtrength of the powder; and let me juſt obſerve, that gunpowder, waſhed from its ſalts and ſulphur by cauſtic alkali, affords the fineſt powder of charcoal for experiments. The nitre is diſſolved in pure water, and the ſolution mixed with the ingredients in wooden troughs, where it is carefully kneaded and incorporated, in form of a thick paſte. It is kneaded, for two or three days, with wooden peſtles, adding a little water as it evaporates. It is granulated by forcing it through ſieves; then ſifted, and the duſt returned into the kneading troughs. Laſtly, it is ſuperficially hardened and ſmoothed, by toſſing it horizontally in ſhallow boxes, which get a briſk reciprocating motion by machinery. The duſt being again ſifted from it, the powder is now fit for uſe. The granulation makes it keep better, and alſo kindle more rapidly in the chambers of fire-arms, by allowing a free paſſage for the flame among the grains.

When gunpowder is fired, and the fluid in a ſtate of incandeſcence, it is as elaſtic as common air would be if compreſſed into $\frac{1}{1000}$th part of its bulk; or is 1000 times more elaſtic than common air. But when it has grown cold again, its bulk is vaſtly leſs, not being 300 times bigger than the gunpowder. *(See Robins's Eſſay on Gunnery.)*

There is another compoſition which explodes with ſtill greater force

than gunpowder. It conſiſts of three parts of nitre, two of fixed alkali, and one of ſulphur. When heated ſlowly on a plate of metal, it firſt melts and becomes black, or of a dark liver colour, and then explodes with a loud crack. Hence it has got the name of *pulvis fulminans*.

The cauſe of its loud and ſmart exploſion muſt undoubtedly be the inſtantaneous converſion of it into elaſtic vapour; not in ſucceſſion, however rapid, like gunpowder, but at once. The effect of this ſimultaneous converſion appears even in gunpowder. This, if fired in a train, makes no noiſe. *(See Note 32. at the end of the Volume.)*

Another ſervice derived from this property of nitre is the production of a brilliant light, fit for making ſignals to be ſeen from a great diſtance. The Chineſe *blue lights* are eſteemed much ſuperior to any made in Europe. A gentleman who made very curious inquiries about their fabrication, and ſaw them made by the Chineſe fire-workers, informs me that the compoſition conſiſts of twenty-eight parts of nitre, ſeven of ſulphur, two of arſenic, one-half of rice-flour, and as much water as will knead them into a ſtiff paſte. This water and the flour retards the inflammation. The paſte is rammed into little earthen pots, and kept in pitched cloths.

Nitre is very eaſily decompoſed, ſo as to obtain either the acid or the alkali in a pure ſtate. It is from nitre only that the nitrous acid is obtained. You have ſeen that mere heat will not give us the acid in a perfect ſtate. We muſt employ ſome intermedium, which ſhall lay hold of the alkali, and allow the acid to riſe from it by the action of heat. Looking at the place of this ſalt in the table of compound ſalts, you ſee at once that the vitriolic acid may be employed for this purpoſe. This acid, having both a ſtronger attraction for the alkali, and leſs volatility than the nitrous acid has, muſt be a very fit intermedium.

The preparation of the nitrous acid is a conſiderable buſineſs, carried on by particular chemical artiſts. As they prepare it for the market, where it is not in general wanted very ſtrong, they uſually ſell it much

diluted with water, and often adulterated with vitriolic acid, which is a much cheaper article. Some arts, however, require it of its utmoſt ſtrength, and very pure. The proceſs for accompliſhing this was firſt diſtinctly given by Glauber, and the acid ſo obtained has got the name of *Glauber's ſpirit of Nitre.*

This proceſs requires certain rules of procedure, and exhibits ſeveral very inſtructive phenomena. The nitre muſt be very pure, and muſt be prepared by melting, and keeping it in that ſtate for ſome time. It is then broken into middle-ſized pieces, and put into a coated glaſs or an earthen retort, already ſet in the ſand-pot for diſtillation. Two-thirds, or three-fourths of its weight of the ſtrongeſt colourleſs ſulphuric acid muſt be poured on it ſpeedily, by means of a crooked or retort-funnel. The receiver is immediately joined to the retort, with fire lute, and then a hole pierced through the luting with a wire. The mixture ſoon grows hot, and, if the nitre had been in powder, or even in very ſmall pieces, the heat might chance to be ſo great and ſudden as to ſplit the retort. The ſalt gradually melts, and acquires a deep orange colour, and ruddy ſteams obſcure the whole veſſels. Theſe ſteams are cauſed by ſmall air-bubbles, which form all over the mixture, and come to its ſurface, where they break. The vile ſmell of nitrous acid now fills the laboratory, proceeding from the vapour which comes through the hole in the luting. This muſt be allowed to continue till it abates. The fire may now be kindled, and allowed to heat the retort very ſlowly. The ruddy vapours ceaſe, and the veſſels grow clear. Let the hole in the luting now be ſtopped. The diſtillation goes on, and the receiver ſoon grows too hot, and muſt be kept cold by ſnow, or by a flannel covering, on which a ſtream of cold water is conſtantly trickling. The fire may now be raiſed conſiderably, and kept up in proportion as we can promote the condenſation in the receiver. When the diſtillation is far advanced, the veſſels again become dim, and ruddy clouds appear. The proceſs is now drawing to a cloſe. The matter in the retort has but little fluidity, and

is very apt to burſt into violent ebullitions, and to drive the contents over into the receiver. But, by due care, the diſtillation may be carried on till the retort is red hot. But, from the firſt appearance of the ruddy clouds, which become ſo thick that we cannot ſee through them, much incoercible vapour muſt be allowed to eſcape. Twenty-four ounces of nitre will require eight hours for the diſtillation, reckoned from the time of kindling the fire.

The acid thus obtained is of the moſt fuming kind, and of a deep orange colour: But, if put into a retort, and a gentle heat applied, and a little water put into the receiver, the ruddy fumes and orange colour will ſoon ceaſe, and the acid in the retort become colourleſs, like water. The ſmall quantity of vapours condenſed in the receiver tinges the water which condenſes them with a deep blue or green. We get here a diluted acid, and, in the retort, the nitric acid in its pureſt form and greateſt ſtrength. It is ſometimes tainted with ſulphuric acid; but it is eaſily freed, by diſtilling it from a little pure nitre. A taint of muriatic acid is more difficult to be removed: We ſhall mention the moſt effectual method as we proceed.

The fuming acid has been called the *nitrous acid*, (*acidum nitroſum*) and the colourleſs acid is called the *nitric*, (*acidum nitricum*). It is intended by this laſt name to denote the origin of the acid, and by the other to denote a redundancy in the principle peculiar to nitre. We ſhall very ſoon ſee the principle on which this opinion of a redundancy is founded.

It is plain that the reſiduum in the retort muſt be the compound of ſulphuric acid and vegetable alkali, perhaps mixed with a portion of undecompoſed nitre. Before a perfect knowledge of the procedure of nature was acquired, it was thought to be a peculiar ſalt, and it got fanciful names; *Arcanum duplicatum; Sal de duobus; Sal enixum, &c.* But it is now perfectly underſtood to be vitriolated tartar. Indeed, this is the only way in which vitriolated tartar is prepared. The ma-

nufacture of nitrous acid furniſhes more than there is any demand for.

Such is the proceſs for obtaining the nitric acid in its ſtrongeſt ſtate. It is much more generally wanted, in various manufactures, in a far weaker ſtate, called AQUAFORTIS, conſiſting of nearly equal parts of Glauber's ſpirit of nitre and of water. In preparing it of this ſtrength, other methods were formerly practiſed. Intermediums, which have ſome, though perhaps not very great, affinity with the alkali, and which are fixed in the fire, are employed; alſo ſubſtances containing vitriolic acid weakly united to ſome other baſe. Of this kind are vitriol, alum, clay, &c. Theſe ſubſtances enable the ſalt to ſtand a great heat, in conſequence of which the volatile ruddy fumes are very copious, and much will be loſt, did we attempt to produce a ſtrong acid. But, by employing much water in the receiver, the ſteams are pretty well condenſed, and a weak acid is obtained, which can be concentrated by another diſtillation *. This difficulty ariſes from the decompoſition, which I have ſaid that the nitrous acid ſeems to undergo, when urged by a very great heat.

And this leads me to take notice of ſome other products of our proceſs for Glauber's ſpirit of nitre, beſides the acid in the receiver, and the vitriolated tartar which remains behind in the retort: I mean the incoercible fumes which eſcape in the beginning and the end of the proceſs.

I have already obſerved that nitre, when urged by a red heat, waſtes, and is rendered alkaline; and yet, that we obtain little or no acid by condenſing the vapours. Nitre melts, then ſimmers, and emits red vapours. Alſo, in our proceſs, ſimilar vapours are produced.

* Mr. Lavoiſier ſays (*Ker's tranſlation of Lavoiſier's Chemiſtry*, p. 215.) that nitre, decompoſed by clay, both yields a ſtronger acid, and with leſs loſs, than by employing the ſulphuric acid.

EDITOR.

There is, however, a very important difference in the two caſes; in the one we obtain acid, and in the other nothing but incoercible vapours. This muſt evidently ariſe from the intermedium, which, by detaching the acid, allows it to come off with a partial decompoſition; whereas, when no intermedium is uſed, the different parts of the acid ſeem to come off in the order of their volatility and attraction.

Notwithſtanding the ingenious experiments of Mayhow, and his important inferences from them, and the no leſs ingenious experiments of Dr. Hales, nearly to the ſame purpoſe, no notice was taken of the air obtained from nitre in thoſe experiments, till about 1772. Dr. Scheele, whoſe experiments with *hepar ſulphuris* I have mentioned to you, began to take notice of this air. He it was who obſerved that a bit of charcoal, held above the red hot nitre, burnt with a dazzling flame. Recollecting his experiments with *hepar ſulphuris*, in which it appeared that only a part of our atmoſpheric air will ſupport flame and combuſtion; and now obſerving that this vapour from nitre ſupported it in an eminent degree, it ſtruck his imagination that this might be the *only* part of atmoſpheric air fit for this purpoſe, and *eminently fit*, becauſe unmixed with the reſt. It immediately occurred to him to try the effect of this air on *hepar ſulphuris*. He expelled it from nitre in a ſmall retort, by a ſtrong red heat, and collected it in a jar. Now putting a quantity of *hepar ſulphuris* into this air, as he had before done into common air, he found that it was *wholly abſorbed*, and this with great expedition. He was now fully convinced that our atmoſphere conſiſted of two airs, one of which ſupported flame, and appeared to him to be a conſtituent part of fire itſelf. He therefore called it FIRE-AIR, which his tranſlators have changed into EMPYREAL AIR.

Much about the ſame time, and without any knowledge of Dr. Scheele's labours, Dr. Prieſtley made the ſame obſervations. Expoſing to a red heat ſubſtances containing, or moiſtened with, nitrous acid, he obtained an air, in which bodies burnt with dazzling flame and

great rapidity. He conceived this to be *pure air*, and that the other portion, which extinguiſhed flame, has been originally the ſame, but that it is now ſaturated with phlogiſton, and cannot ſupport flame by receiving more. He therefore called this depurated portion DEPHLOGISTICATED AIR. Other philoſophers, obſerving that it alſo ſupported animal life, called it VITAL AIR.

Theſe experiments have, of late years, been repeated in a great variety of forms, with the ſame reſults. In particular, it is found that when nitre is expoſed to violent heats, adding a little ſlaked lime, which imbibes the melted nitre, and hinders it from corroding the veſſel, and enables the acid to ſtand a very great heat, the quantity of air obtainable from it is prodigious. Four ounces of pure nitre, treated in this way, furniſhed to Mr. Ingenhouſz 3000 cubic inches, which is about 650 times the bulk of the nitre. Moreover, when we compare its power of ſupporting combuſtion and animal life, with that of common air, it appears that one cubic meaſure of it will be as effectual for either of theſe purpoſes as four or five of common air.

You will recollect that when conſidering the vitriolic acid, and its production from ſulphur, and the reciprocal production of ſulphur from it, I ſaid that it ſeemed neceſſarily to reſult from all our experiments, that, in the inflammation of ſulphur, a quantity of air diſappeared, and that the acid produced exceeded the ſulphur in weight. I alſo obſerved that when ſulphur was produced from ſubſtances containing vitriolic acid, by urging them with violent heats in contact with inflammable ſubſtances, there was always a quantity of aërial matter extricated: And, laſtly, that when *hepar ſulphuris* was expoſed to the air, it was decompoſed, that is, the ſulphur was ſeparated from the alkali. Moreover, this alkali was not recovered in its pure form, but ſaturated with vitriolic acid, of which there was none in the mixture. At the ſame time, air was abſorbed, and the remainder was unfit for maintaining flame. It appears, from a combination of

all theſe facts, that the air thus abſorbed contributed to the formation of the vitriolic acid. *Now* it appears that the air which is abſorbed, is of the ſame nature with that which is emitted by nitre, and is furniſhed by its acid.

From a collective view of theſe facts, it would ſeem that the *ſame* air which is contained in nitrous acid, is alſo contained in the ſulphuric. Farther, as the ſulphuric acid appears to differ from ſulphur in nothing but by the combination with this air, it ſeems to derive its acidity from it: For we find that nothing more is neceſſary for converting ſulphur into ſulphuric acid, but the abſorption of vital air in inflammation, and that it will abſorb it completely. Analogy ſhould lead us, in like manner, to ſuppoſe that the nitric acid derives its acidity from the ſame ſource—from vital air,—and conſiſts of vital air united to a certain baſis, which may be conſidered as the characteriſtic radical of nitrous acid, as ſulphur is of the vitriolic.

Theſe conſiderations, joined to many others of the ſame kind, have induced Mr. Lavoiſier to conſider vital air as the cauſe of acidity, and to call it *oxygen* gas. We ſhall find the proofs of this general property of vital air multiplying on all hands as we advance. There is no acid ſubſtance from which we cannot obtain much pure vital air, and the weight of the air obtained is equal to that loſt by the acid. All compounds, indeed, are not acid, or ſour to the taſte, but all have chemical properties analogous to thoſe of the undoubted acids. I, therefore, admit the propriety of the name oxygenous gas, and ſhall uſe it without heſitation in the reſt of this courſe.

You have heard how the nitre is affected by the ſimple expoſition to ſtrong heat; and the explanation of the change, by the eſcape of the oxygenous gas, is very obvious. But the vapours are not nitric acid; and, therefore, nitric acid does not conſiſt of oxygenous gas alone, but contains it, combined with ſome radical; and this combination is deſtroyed, or decompounded, during the eſcape from the nitre. But,

in the proceſs for nitrous acid, an intermedium is employed, to looſen its combination with the alkali, ſo that it gets off in a more moderate heat without decompoſition. Yet the appearances in the proceſs ſhould make us ſuſpect that this is not altogether the caſe: For the ruddy clouds, at the beginning and end of the proceſs, look very like ſome tendency at leaſt to decompoſition; and the ſituation of things at the end of the proceſs is not very different from that of nitre ſimply expoſed to great heat. It is then almoſt dry, and the retort almoſt, if not altogether, red hot. This did not eſcape the notice of the ſagacious Scheele. But he found appearances which puzzled him. When he held a candle to the ſpout of the retort during the appearance of the laſt clouds, it burnt with a moſt vivid flame, confirming his opinion that the acid was then ſuffering a decompoſition. But, when he did the ſame thing during the appearance of the firſt clouds, the candle was extinguiſhed in an inſtant. This laſt event happens, partly becauſe the portion of oxygenous gas which is now eſcaping is very ſmall, and chiefly becauſe it is accompanied by another ſubſtance in the nitric acid, which has not yet thrown itſelf in our way as one of its conſtituent parts. You will ſoon become acquainted with it. And, in the mean time, I imagine that you are now convinced that vital air is *one* of its ingredients, and that, in all probability, it is the cauſe of its acidity, as it unqueſtionably is of the acidity of the ſulphuric. Mr. Lavoiſier attributes the oxygenous eruption obſerved by Scheele to the ſtrong attraction of oxygen for caloric in high temperatures, which decompoſes the nitric acid.

I may now take notice of another reſemblance in the union of the oxygenous gas, with the characteriſtic ingredients of the two acids. The fuming Glauber's ſpirit of nitre may be rendered ſtill more fuming, by the addition of a very minute portion of inflammable matter. It is obſerved, that, as this quality is increaſed, the activity as an acid is diminiſhed. The ſmall quantity of acid obtained in clearing Glauber's acid of its fumes, is not only weak by dilution, but is weakened

in its attraction for alkalis, and can be diſlodged from them by vegetable acids and ſedative ſalt, without the aſſiſtance of heat. It may be made ſo fuming as to be ſcarcely coercible, and ſcarcely acid to the taſte. Now, when it is converted into nitric acid, it is plain that its proportion of oxygenous gas is increaſed; and when the nitric acid is made fuming, its proportion of the radical is increaſed. It is with propriety therefore called nitrous, (*nitroſum*) as abounding in the characteriſtic ingredient. All this has its counterpart in the ſulphuric and ſulphurous acid; the latter containing more ſulphur, or leſs oxygenous gas, has leſs acidity, and leſs attraction for alkalis.

This proceſs for decompoſing nitre, in order to obtain the acid, has been very inſtructive, as I ſaid it would, opening to our view a new connection of chemical ſubſtances. It has not, however, completely explained all the phenomena, but has rather rendered the ſubject more complicated, by ſhewing that the acid itſelf is a compound ſubſtance, without informing us of its characteriſtic ingredient. I might tell you now that this is nothing elſe than that portion of the atmoſpherical air that is not abſorbed by the *hepar ſulphuris*, or that is left behind by the inflammation of bodies, and may alſo be obtained in a variety of eaſy ways; but I cannot yet demonſtrate this by fairly educing it from the acid, nor confirm the doctrine, by recompoſing the acid by their mixture. We arrive at this by a very circuitous proceſs, which requires the knowledge of ſeveral things with which you are unacquainted. I may, however, mention one fact, of which you can ſee the value without farther acquaintance with chemical ſubſtances. This is an experiment by the Honourable Mr. Cavendiſh, deſcribed in the Philoſophical Tranſactions for 1785 and 1788. He mixed a quantity of oxygenous gas, and Prieſtley's phlogiſticated air, in the bend of a glaſs ſyphon, the two legs of which contained a ſolution of vegetable alkali. When he had, after many trials, obtained the due proportion of the two airs, he found that, by paſſing a ſucceſſion of electrical ſparks through the mixture, they united, and the compound

was abſorbed by the alkaline ſolutions. This, when examined by evaporation to dryneſs, or by cryſtallization, gave a ſmall portion of true ſaltpetre. Hence it unqueſtionably follows that the union of theſe two airs compoſes nitric or nitrous acid. Many queſtions ariſe on this occaſion, which I am not yet in a condition to anſwer.—And, therefore, we ſhall quit the ſubject for the preſent, and proceed with our decompoſition of nitre.

Margraaf firſt mentions the decompoſition of nitre by the muriatic acid. We are not yet in a condition to explain this completely. I can only obſerve at preſent with propriety, that this happens very readily when the nitre has been kept for ſome time in a melting heat. Now, in this caſe, we know that the acid has a ſmaller portion of its oxygen, and that in this condition it has leſs attraction for alkalis. It is for the ſame reaſon, as we ſhall ſoon learn, that this anomaly in elective attraction obtains; namely, in conſequence of the muriatic acid depriving the nitrous of a portion of that principle. (See Bergmann on Elective Attractions, VI.)

If we deſire to decompound the nitre to obtain the alkali alone, we muſt deflagrate with charcoal or ſome vegetable inflammable matter. The common method with charcoal is inconvenient, by being tedious and imperfect. Newman's method is to take ſeven parts of nitre, and one of charcoal. Theſe are pounded, mixed, and projected into a red hot crucible. This method is not bad, but the deflagration is too violent, and ſome alkali is loſt. It is better to moiſten this mixture with a little water, and then to project it into a red hot crucible, and to melt it in the end, and pour it out. I may take this opportunity to remark, that when the chemiſts have occaſion to throw different materials into a heated crucible or other veſſel, in order that they may act on one another, they ſay that they PROJECT the ſubject of the operation, or perform PROJECTION. I ſuſpect that the words projector and project, are derived from this chemical term.

If a ſufficient heat be applied, the alkali is in a melted ſtate at the

end of this operation, and muſt be immediately poured out into a warm mortar, or other veſſel that is warm and dry. The alkali thus obtained, was called *nitrum fixum*, or *nitrum fixatum*. When the operation is properly performed, the quantity obtained is very conſiderable; generally two-thirds, ſometimes three-fourths of the nitre.

The vegetable ſalt tartar is alſo often uſed as a ſubſtance to be deflagrated with nitre, in order to diſſipate or conſume its acid. The acid of the tartar being deſtructible by fire, and inflammable, or containing a quantity of inflammable matter, ſerves in the place of charcoal. And thus we obtain not only the alkali of the nitre, but alſo that of the tartar itſelf. This method, therefore, of obtaining alkali for particular purpoſes is often uſed, becauſe the alkali both of nitre and of tartar is very pure. The common method is to mix and fire them like a ſquib. It is not neceſſary to add water, becauſe tartar contains water and other volatile matters, the gradual diſſipation of which keeps the mixture cool, and hinders the deflagration from going on too violently. The uſual proportion of the ſubſtances is to take equal parts, but this is too little tartar. I find ſix parts of nitre, and ſeven of tartar better for complete decompoſition; though the alkali is not then ſo ſharp and acrid as in the other way, for reaſons to be explained hereafter. Exact mixture is neceſſary. This preparation, however, muſt be diſſolved in water, filtrated, and evaporated to dryneſs, in order to free the alkali from the earthy aſhes of the tartar.

Thus we can ſeparate the two ſalts of which nitre is compoſed, and can ſatisfy ourſelves that the analyſis of it is complete, by joining them together again; by doing which properly we can obtain an entire and perfect nitre again.

Origin.——There are conſiderable quantities of nitre produced in different parts of Europe. But the greateſt quantities of it are imported from the eaſtern parts of Perſia, many parts of China, and alſo from India. The general accounts of its origin in thoſe parts of the world are, that it is obtained from the vegetable mold, or ſuperficial

ſoil in particular places or diſtricts, called *ſaltpetre grounds*, which have generally a looſe, open, and marly appearance.

Theſe accounts appeared for a long time unſatisfactory, and even improbable, as it was not then known that nitre was produced in this manner in any other part of the world. But ſince the ſcience of natural hiſtory has been ſo much improved as it has been of late, ſaltpetre is known to be produced in a ſimilar manner in many other countries, particularly in North and South America, and in Podolia, Walachia, Spain, and Italy.

In Spain and Italy, there are diſtricts in which the land is very fertile, and produces very fine wheat, but it happens frequently in dry ſeaſons that the crop is loſt for want of rain. The farmers then indemnify themſelves by extracting ſaltpetre from the ſoil which ſhould have afforded them wheat.

Much the greater part, however, of what is manufactured in Europe, is got either from artificial compoſts, or accidental mixtures, in which animal and vegetable ſubſtances have been thoroughly rotted, and expoſed long to the air, with a large proportion of any ſpongy and looſe earth, of the chalky or abſorbent kind, as lime, rubbiſh, or marle.

In a part of Poland (Podolia) where the country is an extenſive and rich plain, now deſerted, though formerly very populous, nitre is extracted from the ſoil of little hillocks, which are the remains or ruins of former habitations, and in which the wood of the buildings, and the dung of cattle, have been long thoroughly rotted, and intermixed with rubbiſh and earth. In France too, they extract nitre from the lime and rubbiſh of old ruinous buildings, and from the floors of ſtables and pigeon-houſes. And ſometimes they take the richeſt part of the mold from the ſurface of the common ſoil, in particular places. The ſame practice was common in North America during the laſt war. The earthen floors of their tobacco houſes were alſo found to be very rich in nitre.

Theſe are, therefore, examples of the extraction of nitre from natural or accidental mixtures, in which it is found. To give you, in the next place, ſome knowledge of the artificial compoſts which have been obſerved to afford it, I ſhall firſt recommend to your notice an author upon whom we may fully depend. Cramer adviſes any perſon who deſires to make experiments upon the production of nitre, to chooſe out or build a little hut, expoſed to the freſh air of the country, and covered with thatch, to keep off the rain and ſun, but not otherwiſe cloſe; on the contrary, unleſs the air can come in pretty freely through the walls, it muſt have one open window at leaſt upon the north ſide. It muſt be furniſhed with ſhallow boxes, about one foot deep, for different mixtures; but the only mixture which he mentions, and he ſpeaks of it with the air of one who has tried it, is lime-rubbiſh, garden mold, and aſhes. Theſe are to be lightly moiſtened with urine, well mixed, and placed in one of the boxes to rot and dry. As ſoon as they are dry, they are to be moiſtened afreſh with urine, ſtirring and mixing it well up, to expoſe all the parts to the air, which is found to be of great importance to the production of the nitre. If we thus proceed, always moiſtening and ſtirring while drying, when the mixture is dry, a conſiderable quantity of nitre will be produced; ſo that, in a month or two, each pound of the mixture will hold two ounces of nitre, or one-eighth.

In whatever manner, or in whatever materials, the nitre is produced, the extraction of it from theſe materials is generally performed by a particular ſet of people, who make it their ſole or principal buſineſs, and travel from one part of the country to another in ſearch of materials in which nitre is already produced. After having examined materials of this kind by a ſmall eſſay, and having found them to turn to account, they throw them into a large wooden veſſel, and intermix ſome quick lime and vegetable aſhes. The veſſel has a falſe bottom with holes, which is covered with ſtraw; and, under the perforated bottom, there is another which is water-tight. A ſtop-cock

is placed between theſe two bottoms. After the upper part of the veſſel is full of water, it ſoaks through, diſſolves the nitre in its paſſage, and filtres through the ſtraw, leaving the earth behind. The ſolution is then drawn off, and the water evaporated to ſuch a degree that the nitre cryſtallizes. In this ſtate it is impure, of a brown dirty colour, and mixed with ſome ſea-ſalt, which is always found in the materials affording the nitre. It is ſeparated by cryſtallization, and by quick-lime and aſhes. A brown liquor remains, which does not cryſtallize, and when examined, is found to conſiſt chiefly of ſome lime and nitrous acid.

Thus it appears from this hiſtory of nitre, that it can hardly be counted with propriety among the foſſil productions. It is always found in the earth, it is true, but always at the ſurface. There is no example of its being found at any depth, or the examples are rare, and the circumſtances of them particular. Margraaf and others have found a little in the wells of great cities; the origin of which can be eaſily accounted for. Dr. Withering, in his tranſlation of Bergmann's Sciagraphia, ſays ſome of his friends aſſured him they had got nitre from the waters of ſome coal-pits. But it is highly probable that it had been waſhed down into the coal-pits from the ſuperficial ſoil, by the rain-water ſoaking downwards through the earth. It never occurs in mineral waters. Some authors imagined they found it in theſe; but they miſtook another ſalt for it, and judged of its preſence by the form of certain cryſtals which they had obtained; but the form of cryſtals is never to be truſted. As it is therefore found only at the ſurface of the earth, or not far below it, it has long been a very general opinion that it is depoſited or formed there by the air. Some have ſuppoſed that the air furniſhed at leaſt the principal part of the nitre, to wit, its acid; which, finding in corrupted animal and vegetable ſubſtances the other neceſſary ingredient, the alkali, united with it to form a nitre. Others have ſuppoſed that the air afforded, not the nitrous acid, but vitriolic acid. But theſe opinions have not been

ſupported by experiments. When pure alkali is expoſed to the air, it is not changed into nitre.

Another idea which occurred to ſome of the chemiſts in France had a greater appearance of probability; I mean, that the nitre has its origin in vegetables, being produced in them by the powers of vegetation, and is only extricated from their other principles by putrefaction, during which natural proceſs all the principles of animal and vegetable ſubſtances are gradually ſeparated from one another. Many plants have been long known to contain ſome nitre in their juices, ſeparable by cryſtallization. Tobacco ſometimes contains ſo much, that its ſtalk burns as a ſquib; but they were ſuppoſed to have received this from the ſoil in which they had been nouriſhed. It is now reported, however, that ſome of thoſe plants, though made to grow in earth from which all ſaline matter has been completely extracted, and, though watered with pure water only, will ſtill be found to contain nitre; which, if true, ſhews plainly enough that the plant generates the nitre, by combining ſome of the elements together in a particular manner.

Many other opinions were formed on this ſubject at different times, but we never acquired a deciſive and ſatisfactory knowledge of the formation of nitre in ſoils, and other mixtures, till the French government directed to it, in a particular manner, the attention of the able chemiſts of that nation. This was done under the adminiſtration of that patriotic miniſter Mr. Turgot. Diſtinctions and rewards were offered to thoſe who ſhould beſt illuſtrate the origin of nitre; and the Royal Academy of Sciences were appointed judges of the merit of the attempts.

A number of eſſays were accordingly offered, many of them deſcribing ingenious inveſtigations, and trains of experiments, to decide different queſtions relating to this ſubject. The gentlemen who diſtinguiſhed themſelves the moſt are M. Thouvenel medecin, and M. Thouvenel commiſſaire, des poudres et ſaltpetres de Nancy; Mr. Lorgna; M. Chevraud; M. Gavinet. An abſtract of their diſcoveries has been pub-

liſhed by the academy, with ſome remarks, to ſhew what particulars have been cleared up and decided, and what ſtill remain obſcure. It appears from the whole, that nitre is formed by the action of the atmoſpherical air, and of the exhalations of rotting animal and vegetable ſubſtances, applied at the ſame time to alkaline earths, as chalk, or lime-rubbiſh, or marle; and that the putrid exhalations do not produce their effect, except atmoſpheric air be allowed to mix with them in certain quantity. It appears further, that the nitrous acid is formed moſt readily, or ſooneſt, and the alkali afterwards. The acid firſt joins with the alkaline earth; the alkali which is formed afterwards diſſolves the union, by taking the acid to itſelf.

Several other particulars have been diſcovered, which will be uſeful and inſtructive to thoſe who have the conduct of ſaltpetre works. But they are only facts which point out the methods by which ſaltpetre may be produced with the greateſt ſucceſs. They do not yet explain its production. Other experiments ſince that time have thrown much light on the nature and conſtituent parts of the acid of nitre, and have occaſioned the contrivance of new methods of producing it and nitre. But of theſe we ſhall ſpeak more fully hereafter.

Uſes.——Nitre is an article of the materia medica, and is reckoned the moſt cooling antiphlogiſtic of the ſalts; but it is often very diſagreeable to the ſtomach. It alſo promotes the ſecretion of urine. In the arts it is highly uſeful, by being the only ſubject from which we can get the nitrous acid; and it is by means of nitre that the vitriolic acid is got in quantity or perfection from ſulphur.

SPECIES IV.—CUBIC NITRE.

The cubic nitre reſembles common nitre exactly in its moſt ſtriking properties. The only difference is the form of its cryſtals and the conſequences of decompoſing it. The form which its cryſtals effect, is that of oblique parallelopipeds; though they generally concrete toge-

ther, and adhere to one another, ſo that hardly any of them have this form perfect. It melts with the ſame heat as common nitre, and diſſolves in the ſame manner in water. It deflagrates with inflammable ſubſtances preciſely in the ſame manner, and is decompoſed by the ſame means, viz. by vitriolic acid. The only difference is, that Glauber's ſalt is produced inſtead of the vitriolated tartar. It is decompoſed by deflagration with charcoal for the alkali, which is not the vegetable alkali, as in the caſe of common nitre, but the foſſil alkali. Therefore it is improper to uſe tartar for the deflagration; but we muſt employ charcoal with water, as the charcoal of fir (as Margraaf directs), or we may take common flour, which contains a ſufficient quantity of inflammable matter, and enough of water and other volatile ingredients, to keep the rapidity of inflammation within due bounds. When deflagrated with ſulphur, it is plain that Glauber's ſalt muſt be produced.

It remains now only to mention the origin of cubic nitre. It is not produced naturally in a quantity worth notice, (though I ſuſpect it is contained in ſome vegetables), but is prepared artificially by chemiſts, when they have occaſion for it in their experiments; for it is not employed in medicine or in the arts. It may be obtained either by decompoſing common ſalt with the nitrous acid, as ſhall be noticed hereafter, or by joining together the alkali of ſoda and the nitrous acid.

SPECIES V.—DIGESTIVE SALT.

As digeſtive ſalt is the produce of art only, and is prepared by mixing its two ingredients, the knowledge that we have already acquired of theſe will naturally ſuggeſt to us all its general properties, and the manner of decompounding it for either of its ingredients.

Its external appearance ſo much reſembles that of common ſea-ſalt, that chemiſts of eminence have imagined it to be the ſame. Boerhaave calls it *ſal marinus regeneratus*. The London Diſpenſatory avoids deciding, by calling it *Spiritus ſalis marini coagulatus*; and even the

author of the New Diſpenſatory was long before he imagined it any how different. It differs, however, from ſea-ſalt, in becoming a much more tranſparent maſs after fuſion, but leſs white, and toughiſh like horn. Even the cryſtals have this property, and are pounded with difficulty. Its moſt remarkable difference from ſea-ſalt is, that it diſſolves in much greater quantities in hot than in cold water; and, by this property alone, theſe ſalts may be pretty well ſeparated. Its analyſis is already foreſeen. It may be decompoſed for its acid, by employing the vitriolic acid, and the reſiduum muſt be a vitriolated tartar inſtead of a Glauber's ſalt. I was perfectly convinced of the difference, by decompoſing it with the nitric acid. The reſiduum ſhot into hexagonal cryſtals; a ſufficient proof that it conſiſts of the muriatic acid and the vegetable alkali. To decompoſe it for its alkali, we muſt follow the proceſs, and deflagrate the nitre we have obtained by it.

The only other origin of digeſtive ſalt, is the decompoſition of ſal ammoniac by means of potaſh. This being the cheapeſt proceſs for obtaining it, is practiſed by the trading chemiſts for ſupplying the druggiſts and experimenters with all of it that is wanted.

SPECIES VI.—COMMON SALT.

Common ſalt requires a full red heat for its fuſion, and, ſoon after fuſion, begins to evaporate in white fumes. It has a pretty ſtrong attraction for water, ſo that it deliquiates in moiſt air. It diſſolves in leſs than three waters, and equally in cold or hot. Hence it has a different manner of cryſtallizing by evaporation. A pellicle is formed, if the evaporation be haſty; if ſlow, cubes are firſt formed, then a ſort of hollow pyramids, formed by ſteps, ſuperadded on all ſides to the ſtep laſt formed. The firſt cube, formed at the ſurface, being a very little heavier than the water, begins to ſink, and the adheſion, or viſcidity of the water, cauſes it to drag the adjoining water down with it; and

thus it makes a depreſſion on the ſurface, as when a ſquare brick is laid on a feather bed; only the ſaline cube is wholly under water, hanging by its upper ſurface. The water being thus expoſed to evaporation in a greater ſurface in the ſides of this pit, the next cryſtallization takes place there, preferably to any other place. Thus a firſt ſtep is formed, and the ſolid ſinks deeper, again hanging by its upper edges. A ſecond ſtep is formed in the ſame manner, and ſo on, till the cryſtal becomes too heavy, and it then breaks its hold and ſinks to the bottom. It is amuſing to obſerve this operation with a magnifying glaſs. Each ſtep is formed in an inſtant all round,—yet it conſiſts of diſtinct cubes; for it will only break into cubes, or cluſters of cubes.

The cryſtals of ſea-ſalt are of the decrepitating kind. If it be frequently diſſolved, evaporated, and dried thoroughly with red heat, it is diminiſhed in quantity, and a portion of earth is produced from it. And this is one of the chief inſtances adduced to prove that ſalts are compoſed of earth and water. But the quantity of earth is ſo ſmall, that its origin is doubtful.

Common ſalt is eaſily volatilized by fire, and by putrefaction, along with ſubſtances diſpoſed to that proceſs. By repeated ſolutions and calcinations, it may be totally deſtroyed, or converted into earth.

Although its acid be very volatile, this ſalt cannot be decompoſed by heat alone. In ſome of the proceſſes by which it was attempted formerly to prepare this acid from common ſalt, they truſted principally to the force of heat. Common ſalt, mixed with clay, and expoſed to a fierce heat, is in part alkalized; but it afterwards becomes neutral by expoſure to the air.

The beſt way of decompoſing it for its acid, is by another acid, of which there are two that will anſwer the purpoſe,—the vitriolic acid, and the nitrous; but the vitriolic is beſt, and is even neceſſary if we deſire to have the muriatic acid pure. It is beſt, as having greater ſuperiority of attraction for the alkali than the nitrous acid has, and there-

fore expels the muriatic acid more readily. Indeed this acid is in a manner neceſſary for obtaining the muriatic acid pure, becauſe it is fixed, and does not emit any fumes; whereas the nitrous acid emits them very copiouſly. Further, in uſing the vitriolic acid, it is neceſſary to uſe likewiſe a certain quantity of water. If the ſtrong acid be added to the dry common ſalt, the muriatic acid is expelled ſo pure, that it is not accompanied with water ſufficient for its condenſation; ſo that a great part muſt be allowed to eſcape, for the fumes would burſt the veſſels if we attempt to confine them; and thus we loſe much of the acid, and are incommoded too with the fumes; becauſe the water contained in the cryſtals of the ſalt, and in the vitriolic acid, is too little to repreſs ſufficiently the volatility of the muriatic acid thus detached. We muſt therefore dilute the vitriolic acid with about an equal quantity of water, before it is added to the ſalt in the retort.

With all our care and ſkill, the ſeparation of the muriatic acid is a very difficult proceſs. In its pure ſtate, it is too volatile to aſſume the liquid form, even in our moſt intenſe colds. Dr. Prieſtley reckons it a ſpecies of air, calling it marine acid air. But it ſeems to differ from the other acids, chiefly by its ſmaller attraction for water, of which it requires a greater quantity to repreſs its volatility, and alſo by the very great quantity of heat which its vapour holds latent. If we proceed with a ſimple retort and receiver, we may uſe nearly equal weights of ſalt and vitriolic acid, and dilute the acid, before mixture, with as much water: And we muſt employ a large receiver, and keep it very cool. The tubulated retort containing the ſalt, being ſet in the ſand-pot, and the receiver joined and luted, the diluted acid is then poured in, and the hole ſhut with a ground ſtopper. The diſengagement of the marine acid commences immediately, even though the fire has not been kindled. Some chemiſts think it better not to dilute the vitriolic acid, but to put the water into the receiver, where it abſorbs the vapour very faſt, and ſoon grows extremely hot. The diſtillation muſt be conducted very ſlowly, and it is proper to let it proceed,

for a while in the beginning, without any fire. As we muſt uſe a porous lute, much vapour eſcapes, with great loſs of acid, and much inconvenience from the fumes, which corrode every thing in the laboratory. It is chiefly in this proceſs that the great value of Mr. Woulfe's apparatus, deſcribed in the Phil. Tranſ. 1767, appears, and it is ſo employed by all the trading chemiſts. Mr. Woulfe found that the condenſation of about four ounces of vapour heated ſeven or eight pounds of water up to the boiling temperature. When the water has acquired an equal weight of acid vapour, its bulk is increaſed in the proportion of two to three. I recommend to your peruſal the diſſertation juſt now mentioned, by Mr. Woulfe, as containing many very curious as well as uſeful obſervations. I obſerve that ſome foreign chemiſts proceed in another way that is pretty ſingular. Two retorts are employed, communicating with one receiver. In one of them the diſtilling ſubſtances are contained, and the other has only water. The two vapours thus meet in the receiver, and unite very rapidly. I doubt not but this proceſs will be very manageable and productive.

The ſalt which remains in the retort when the diſtillation is finiſhed, is Glauber's (as may be ſeen from the table); and this is the uſual manner in which Glauber ſalt is prepared by chemical artiſts, the proceſs being performed much oftener for the ſake of the Glauber ſalt than of the muriatic acid; except in ſome manufactories, in which they employ the muriatic acid to make ſal ammoniac.

If we deſire to decompound common ſalt to obtain the foſſil alkali pure, we cannot have recourſe to inflammable ſubſtances here, as in the caſe of vitriolic and nitrous ſalts. When the experiment is made with common ſalt, it does not ſucceed; nor is there any reaſon to expect that it ſhould. The muriatic acid, in its ordinary ſtate, is very little affected by inflammable bodies, and will not quit the alkali to act upon them. But we can ſeparate the acid by the vitriolic or nitrous acids, which, when joined to the alkali in its place, will not be

ſo difficult to remove. The nitrous acid is the moſt convenient, and moſt eaſily managed.

When the nitrous acid is employed to ſeparate the muriatic, a cubic nitre is formed, and it is eaſy afterwards to deflagrate with charcoal to obtain the foſſil alkali, which is the pureſt alkali we have. But the firſt part of this proceſs requires a little attention to perform it well, and Mr. Macquer's directions are very bad. The nitric, and not the nitrous acid, ſhould be employed, and in a diluted ſtate, or about the ſtrength of aquafortis. This muſt be poured on the common ſalt, in fine powder, but retaining its water of cryſtallization. The diſtillation of the muriatic acid ſhould be conducted very ſlowly. The firſt fumes are ruddy, and condenſe into a ruddy liquor, and the fumes of mixed acid, or aqua regia, are very perceptible in the laboratory. Towards the end the veſſels grow clear, and the liquor is yellow, and floats on the ruddy liquor. If the whole be returned and ſlowly rediſtilled, we have leſs of the ruddy fumes and liquor.

The acid obtained from a pound of nitre, of whatever ſtrength, will completely decompoſe about eight ounces of common ſalt. The cubic nitre is eaſily ſeparated from the undecompoſed ſalt by cryſtallization, becauſe it diſſolves in much greater quantity in hot than in cold water. The earthy matter of the charcoal with which it is deflagrated will ſeparate by the filtre, and leave the ſoda perfectly pure.

Common ſalt is well known to be the moſt univerſally uſeful, and the moſt neceſſary to mankind of any. The qualities by which it is ſo uſeful are its antiſeptic power, attended, at the ſame time, with a wholeſome or innocent quality with regard to our conſtitution; in conſequence of which qualities, we can, in caſe of neceſſity, preſerve our animal food in wholeſome condition, for a long time, by means of this ſalt. And it is further much more extenſively uſeful and valuable, in conſequence of another quality, by which it aſſiſts or promotes the digeſtion of our food, and proves a moſt agreeable ſea-

ſoning to it. It is well known to be, on this account, one of the neceſſaries of life with regard to man; and there is reaſon to think it neceſſary or uſeful to the greateſt number of other animals alſo. In theſe iſlands, and in the greateſt part of Europe, which is not far from the ſea, as well as in other ſimilar ſituations, this truth is not very obvious. We think, on the contrary, that we ſee numbers of animals live and thrive very well without ever taſting ſalt. But they do, however, get ſalt, though in an imperceptible manner. Our moſt inland ſituations in theſe iſlands are at ſuch moderate diſtance from the ſea, that ſome ſalt is communicated to them, either by the finer part of the ſpray, daſhed into particles perhaps too ſmall for ſight, and which muſt neceſſarily fly in the air afterwards for a very long time, or by an evaporation of the ſea-ſalt in ſmall quantity along with the water *. In whatever manner ſea-ſalt ariſes into the atmoſphere, and to the upper parts of the land, it is certainly found there. A ſmall quantity can be obtained from our pureſt and freſheſt natural waters. But it does not equally reach all countries and all ſituations. There are ſome far removed from the ſea, or other large collections of common ſalt, in which the animals plainly languiſh for want of it, and ſhew very ſtrong deſire to have it. In Germany, and ſome other parts of the great continents of Europe and Aſia, they give it to their cattle, as neceſſary to their health and thriving. In the inland parts of America, the wild animals are obſerved to flock in incredible numbers, and from great diſtances, to places where they may have an opportunity of licking ſalt, or of drinking ſalt water. Salt is often uſed there as bait for deer, to entice them into places where the hunter can reach them; and an offer of ſalt is a

* I have frequently obſerved at Glaſgow, which is a great way from the ſea-coaſt, that, after a ſtrong gale from the weſt, the hedges, in winter, are very ſalt to the taſte. One would imagine that the twigs had been dipped in ſalt water. One very dry ſpring, I found it cryſtallized like hoar-froſt.

EDITOR.

greater temptation to horſes running looſe in the American woods, than an offer of corn. Theſe facts have occaſioned ſome ſpeculations concerning the manner in which this ſalt can be ſo uſeful to animals in general; and a diſcovery made by the late Sir John Pringle has been thought to throw ſome light on this matter. Sir John diſcovered, by experiments, that it has a power of promoting the putrefaction of animal and vegetable matter, when applied in ſmall quantities to thoſe ſubſtances. And, as the change which the vegetable food undergoes, in paſſing through our bodies, is ſuppoſed to have ſome analogy with putrefaction, it has been thought that common ſalt had the power of promoting this change as well as putrefaction. Animals that are purely carnivorous do not agree with ſalt added to their food. In whatever manner it acts, its uſefulneſs to animals in general is ſufficiently evident.

Origin.——As, upon theſe accounts, common ſalt ſeems the moſt important and the moſt extenſively uſeful of the ſalts; ſo it is moſt abundantly produced by nature. For, firſt, it is found conſtituting deep and extenſive maſſes in the bowels of the earth, from which it is dug up by mining, as coal or other minerals, and is called rock-ſalt. Many mines of this kind are wrought in England, Muſcovy, Poland, Germany, Calabria, Tranſylvania, and Stower in the Upper Hungary, Spain, particularly Catalonia, Italy, and other parts of Europe, in the Eaſt-Indies, and in America. Salt, as thus found in mines, is a ſolid hard ſubſtance, and is not diſſolved ſo readily as ſea-ſalt. It is generally more or leſs tranſparent, but tinged to a brown, yellowiſh, or reddiſh colour, in conſequence of the mixture of ſome earthy matter with the ſalt. Sometimes it is pretty bright and pure; whence it has got the name of *ſal gem.* Many of thoſe mines contain amazing quantities of ſalt. The moſt aſtoniſhing are ſome Poliſh and Hungarian mines, of which there is an account in the Philoſophical Tranſactions, No. 61. and 413. The mine of Cracow in Poland, near Wieliczka and Bochna, is computed to hold ſalt enough to ſuffice the whole world

for many thouſands of years. The works in it are of great extent, and there are houſes, chapels, and other buildings under ground, all built of ſalt or ſalt ſtones. Very large maſſes are often met with, free from conſiderable flaws, and of more pleaſant colour, and more tranſparency than ordinary, which are cut and turned into pillars and other ornaments of architecture,—ſometimes into toys. And the whole of theſe mines, as illuminated artificially, have a moſt uncommon, ſurpriſing, and brilliant appearance.

Theſe immenſe and maſſy collections of ſalt are found, in ſeveral parts of the world, to project above the level of the ground, and form hills and even mountains. Of this kind are two mountains in Ruſſia, near Aſtracan; ſeveral in the kingdoms of Tunis and Algiers, in Africa; ſeveral in Aſia; and a great part of the iſland Ormus, in the Perſian Gulf, is ſaid to conſiſt of ſalt.

There are likewiſe, in different parts of the world, innumerable ſprings, ponds, and lakes, containing this ſalt, produced by the flowing of water through foſſil ſalt, or ſoaking through ſalt earths. Theſe waters are of very different richneſs. Thus, in Germany, where they have moſt of their ſalt from ſprings, ſome are ſo weak that there are only two drachms of ſalt in the pound of water. The ſpring at Halle is the richeſt, and contains three ounces and three drachms to the pound; but in England there are pits much richer than theſe. The ſprings of Droitwich contain four ounces; and ſeveral pits at Northwich, and at Barton in Lancaſhire, contain ſix ounces in the pound, which is a ſaturated ſolution.

Laſtly, The ſea, which covers ſuch a large proportion of the ſurface of this globe, contains this ſalt in very conſiderable quantity, and renders it eaſily procured for the uſes of man, in almoſt all the inhabited parts of the world.

Theſe are the only, though very plentiful ſources from which ſalt is obtained,—the maſſes or ſtrata which it forms in the earth; the ſalt ſprings or fountains, and the ſea. And it has therefore been often di-

vided into theſe three ſpecies,—of rock-ſalt, or ſal gem,—fountain-ſalt, —and ſea-ſalt. But this diviſion is of no uſe, becauſe, from whichever of theſe ſources it is obtained, if it be pure naturally, or purified by art, it is preciſely the ſame. All the differences found among the varieties of common ſalt, in its native ſtate, proceed from impurity or admixture, which produces great variety even in common ſalt of the ſame origin; it being ſeldom or never perfectly pure, and often very foul. Rock-ſalt, for example, is commonly attended with a large intermixture of earthy matter, and has beſides a bitteriſh taſte, proceeding from a ſmall proportion of other ſalts intimately blended with it. Salt of ſprings or fountains is liable alſo to admixtures of mud, and other ſalts. And in the ſea, we find ſeveral other ſalts or ſaline compounds mixed with the common ſalt. The great quantity of water too in which ſea-ſalt and ſpring-ſalt are diſſolved, muſt be conſidered, in a commercial view, as an extraneous matter, which it is neceſſary to ſeparate from it. The proportion of this water to the ſalt is different, however, in different ſprings, and in different parts of the ocean. In general, the ſea is leſs ſalt in cold climates, near the poles, and ſalter towards the equator. It is a common obſervation of naturaliſts, that ſuch is the arrangement and regularity of nature, that ſince this ſalt ſeems mixed with the ſea, in order to prevent its putrefaction, ſo it is mixed in the greateſt quantities in thoſe places where the heat is greateſt, and where, for this reaſon, there is the greateſt danger of putrefaction. This variety of ſaltneſs in the ocean is produced by greater or leſs evaporation, and more or leſs rain. The evaporation is much greater near the line, from the greater force of the ſun; and the vapours, or the greateſt part of them, generally move towards the poles, ſo as to deſcend in more frequent ſhowers there than where they were elevated. And though the difference of ſaltneſs occaſioned in this way is hindered from going a great length, by the agitation to which the ocean is ſubject, from various cauſes, ſtill, as theſe operate ſlowly, the difference is always conſiderable.

Thus, in the northern parts of the Baltic, a pound of ſea water ſcarcely contains two drachms of ſalt. Farther ſouth, from the mouth of the Elbe to Holland, and on the Britiſh coaſts, it contains one ounce. In the Mediterranean it contains two ounces; and in the Atlantic, near the line, it contains more than three. Mr. Boyle has obſerved a phenomenon which he aſcribes to this cauſe. Where the ſea is very deep, the water is ſalter at the bottom than at the ſurface. Even in moderate depths near the land, this inequality of ſaltneſs obtains alſo. This, in all probability, is owing to the freſhes from the land, which float a-top by their ſpecific levity; and it is a long while before they are mixed by the agitation of the winds. Ovid has preceded Mr. Boyle in this obſervation, and deſcribes it moſt accurately in ſome very beautiful lines in one of his elegies.

Theſe are, therefore, the different ſtates in which common ſalt is found in nature, and from which it is prepared and refined for uſe.

Where the rock-ſalt occurs in a ſtate of conſiderable purity, it is uſed by many without preparation. In Poland, numbers of the inhabitants uſe their foſſil ſalt in its natural ſtate; but this is uncommon upon the whole. The rock-ſalt is generally ſo impure that it cannot be uſed, without being firſt refined. And in general, mankind almoſt univerſally uſe common ſalt that has been prepared or manufactured by extracting it from the waters of the ſea, or of ſalt ſprings or wells, or by refining or purifying the common ſalt.

The methods which have been contrived, in different places, for extracting it from the waters of the ſea and ſprings, with the leaſt expence, are very numerous; and much ingenuity has been ſhewn in purſuit of this object. But I believe I can employ your time better than in deſcribing all theſe contrivances. You will find a very good account of them in Dr. Brownrigg's book upon ſalt, intitled, "The Art of Making Salt."

I ſhall only remark, that all the methods which have been in uſe may be diſtinguiſhed into two varieties, and a mixture or combination

of theſe two: 1*ſt*, In the one, the water is evaporated, and the ſalt made to cryſtallize by the heat of the ſun and action of the air only, in ſhallow ponds. 2*d*, By the other, theſe operations are performed by the heat of fuel; and the ſalt is called boiled ſalt.

The firſt method is practicable in the hot climates only, where the great heat of the ſun, and dryneſs of the weather in ſummer, occaſion a copious and quick evaporation, without any expence. In thoſe climates and ſituations where theſe circumſtances are the moſt favourable, very little art is required, or ſcarcely any; for wherever the ſea-water is received at ſpring tides into ſhallow ponds or pools that are not overflowed at other times, the ſalt cryſtallizes between one ſpring-tide and another. But where the evaporation is not ſo quick, more art is employed, as in the ſalt ponds on the coaſt of France. The general contrivance is to this purpoſe:—On a flat beach, the ſea is walled off, except at one entry, which is ſhut by a ſluice: All within it is the ſalt pond. This is divided into a vaſt number of portions, in the manner of a labyrinth, all communicating with the main inlet; but by ſo many turnings, that any one trench, if ſtretched into a ſtraight line, is very long, and all of them nearly of the ſame length. The different portions are made ſhallower as they advance inland. When the ſea is admitted at high water, the whole is filled to a level, and the ſluice is ſhut. The evaporation reduces the ſurface. At next high water, more is admitted: this puſhes before it the water already in the pond, and now a little ſalter than what is now admitted. Thus we ſee, that as the brine becomes richer in ſalt, it is puſhed farther in, toward the laſt receſſes of the labyrinth; and therefore the granulation will begin in thoſe places, and the water will continue to depoſit its ſalt chiefly there, where it is lifted out with proper inſtruments. Theſe parts of the ſalt pond are, therefore, firmly conſtructed, and formed of ſtiff clay, well rammed; while the parts nearer the inlet are of common earth, or frequently of mere ſand.

The ſecond method of preparing ſalt, viz. by the heat of fuel, is practiſed in theſe colder and wetter climates, which do not admit of the firſt. In this country eſpecially, the preparation of boiled ſalt is a conſiderable buſineſs, becauſe we have fuel very plenty and cheap; and a part of it may be had for almoſt nothing, on account of its being unfit for houſehold purpoſes.

In ſome places in England, they practiſe both methods in part; that is, they evaporate part of the water, firſt by the heat of the ſun, and afterwards finiſh with fuel.

The ſalt made with the ſun and air alone, is called bay-ſalt. Its cryſtals are much larger, and more ſolid than thoſe of boiled ſalt, as it is commonly made, but not ſo white, by reaſon of light mud which remains ſuſpended during the whole evaporation. But the bay-ſalt, though it has the appearance of being more impure, is much more effectual for preſerving proviſions; and, when we make an inquiry into its conſtituent parts by experiments, and comparing with boiled ſalt, we diſcover the reaſon of this, which is, that it is in reality purer than the common boiled ſalt; that is, it is more free from the admixture of other ſalts, or ſaline compounds. For, as I remarked already, ſea-water contains ſeveral ſalts, or ſaline compounds, beſide common ſalt, and theſe give it the bitteriſh taſte. By the violent boiling, and haſty cryſtallization commonly practiſed in preparing boiled ſalt, theſe extraneous and uſeleſs ſalts are but very imperfectly ſeparated. A great part of them remains, and depraves the quality of the ſalt for curing proviſions; whereas the bay-ſalt being produced by a ſlow evaporation and cryſtallization, there is more time for the ſeparation of the heterogeneous, and concretion of the homogeneous ſaline particles. That this is the cauſe of the difference, appears from Sunday's ſalt, which remains upon a ſlow fire that is not renewed during the whole of that day, and its cryſtals are much larger, more agreeable to the taſte, and ſell for a higher price. Bay-ſalt has a taſte very ſenſibly ſweetiſh, which is not to be found in any boiled ſalt whatever, and

which it loſes by two or three ſolutions and cryſtallizations. It is in all probability owing to the influence of the ſun's light, the chemical effects of which on other ſubjects are analagous to this.

It is, however, very much to be wiſhed, and an object of great conſequence to trade and the fiſheries, to prepare common ſalt as free as poſſible from all impurities; and many ingenious men have turned their attention to this ſubject. It is the main ſcope of Dr. Brownrigg's book, though he was perhaps under a miſtake in his notion of the defect of common boiled ſalt.

The Earl of Dundonald has lately publiſhed the account of a cheap, though ingenious proceſs, by which common boiled ſalt can be purified to a very conſiderable degree from the extraneous ſalts. The general principle of his proceſs is this: The diſagreeable ſalts contained in the common boiled ſalt, diſſolve in much greater quantities in hot than in cold water. In order, therefore, to purify a parcel of common ſalt, it is put into a conical veſſel, or baſket, having an aperture at its apex, ſtopped with a plug of ſtraw. A ſaturated ſolution of common ſalt in boiling hot water, is poured upon it, boiling hot. This trickles down through the ſalt, and eſcapes through the ſtraw. Being already ſaturated, it can carry off none of the pure ſea-ſalt, but it waſhes it clear of the bitter brine with which it was covered; and, being boiling hot, it diſſolves the greateſt part of the cryſtals of the bitter purging ſalt which had tainted the whole. His Lordſhip found, by accurate experiment, that a ſaturated ſolution of one pound of common ſalt, poured upon ten pounds of ſalt, takes away about four-fifths of all the bitter ſalts that it contains, by one operation.

My friend the late Dr. Roebuck alſo ſtudied this ſubject, and practiſed a proceſs, by which he refined boiled ſalt to a very high degree, and made it even ſuperior to bay-ſalt. But his proceſs was expenſive, and cannot be made ſubſervient to the fiſheries, but only to ſupply the luxury of the rich. It depends entirely on repeated and ſlow cryſtallization.

It is much to be wiſhed, that the preparation of common ſalt, and the commerce in it, were leſs ſhackled with limitations, monopolies, and heavy duties. It is not only a neceſſary of life to all ranks, but of the greateſt conſequence to the fiſheries: and ſalt is capable of being employed in a number of manufactures, which are prevented from taking place by the price of this article, and by the trouble to which thoſe who uſe it are ſubjected.

AMMONIACAL SALTS.

All the compound ſalts which contain the volatile alkali combined with the different acids, are called ammoniacal ſalts. This general denomination is taken from the compound with the muriatic acid, which is commonly uſed, and has been long known by the name of ſal ammoniac. The name is derived by ſome from Ammonia, one of the Cyrenaic territories, in Egypt and Lybia, which was ſuppoſed to produce it. Pliny, in treating of common ſalt, ſays, that the Cyrenaic territories were remarkable for producing a ſpecies of it called Hammoniac, on account of its being found under the ſand of the ſoil; and it is ſuppoſed that he meant this ſalt, which is very probable, both on account of the name, and of the deſcription which he gives of it; but it is plain that he had only an imperfect knowledge of its nature, and had been miſinformed concerning its origin. *Vide Plin. Hard. tom. II. p.* 559.

With reſpect to the general nature of theſe ſalts, they can always be diſtinguiſhed from the compound ſalts, by the action of a fixed alkali on them; it immediately attaches itſelf to the acid, and detaches the volatile alkali, which is perceptible by its pungent odour. It is by the decompoſition of one of theſe ſalts that we obtain a pure volatile alkali, or that which is the fitteſt for chemical experiments, or for

the purpoſes of medicine. If equal quantities of ſal ammoniac and ſalt of tartar be diſſolved in an equal weight of water, we ſhall immediately perceive the pungent ſmell of the volatile alkali. If this mixed ſolution be put into a retort, and diſtilled to dryneſs, with a heat gradually increaſed, we ſhall find in the receiver a ſolution of volatile alkali, in its leaſt acrid form. It is known by the name of *ſpirit of ſal ammoniac* or *of hartſhorn.*

Beſide this effect of the fixed alkali on them, which diſtinguiſhes the ammoniacal ſalts, they are alſo more volatile than other compound ſalts,—both of the ſimple ſalts of which they are compoſed being very volatile. But their volatility is not ſo remarkable as we might be led to expect from our knowledge of the volatility of their ingredients in a ſeparate ſtate.

SPECIES VII.—VITRIOLIC AMMONIAC.

The firſt in the table is the *Vitriolic Ammoniac*, likewiſe called the Secret Ammoniac of Glauber, becauſe that author, who was inventor of Glauber's ſalt, was likewiſe the firſt who took particular notice of this, and imagined that it poſſeſſed ſome remarkable powers over metals, and made a ſort of ſecret of it for ſome time. But I do not know of any qualities which it poſſeſſes that deſerve your attention.

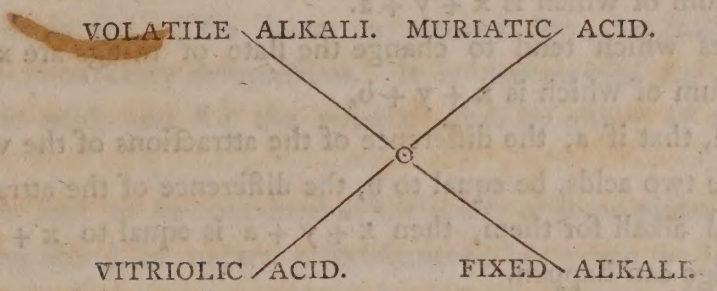
We may, however, before we diſmiſs this ſalt, take the opportunity to explain a double elective attraction in which it plays a part. The fact, as diſcovered by experiment, is this—

If vitriolic ammoniac and common ſalt be both diſſolved in water, and mixed together, and the mixture boiled for ſome time, there is a double exchange, or double elective attraction. The vitriolic acid leaves the volatile alkali, and unites with the fixed alkali of the common ſalt, while, at the ſame time, the acid of the common ſalt

unites with the volatile alkali that was in the vitriolic ammoniac; and then, by evaporating the ſaline mixture, we obtain firſt Glauber's ſalt, and afterwards common ſal ammoniac *.

The reaſon of this muſt be, that there is a greater difference between the attractions of the fixed alkali for the two acids, than between the attractions of the volatile alkali for the ſame two acids, which may be ſhortly expreſſed by ſaying that the fixed alkali has a greater partiality for the vitriolic acid than the volatile alkali has. For, let us ſuppoſe that theſe differences are the ſame, and then conſider what would be the conſequences.

Theſe four chemical ſubſtances, the two alkalis and two acids, when mixed together, may be repreſented, in ſome meaſure, by four bodies placed at the extremities of two moveable diameters of a circle, and each of them attracting the two that are next to it; thus,



Each of the acids is diſpoſed to unite with either alkali, and each of the alkalis conſequently with either acid. But they cannot unite with one without leaving the other: and in proportion as they are

* It is eaſy to perceive that the double exchange will happen without the condition required by Dr. Black, if the partiality of the vitriolic acid for the fixed alkali be greater than that of the muriatic acid for the ſame alkali. This operation muſt be conſidered in the more general manner expreſſed in note 24. p. 280.

EDITOR.

attracted by one, they are drawn away from the other; and while one of the acids unites with either alkali, it repels the other acid from it, and in the ſame manner either alkali repels the other. Now let us ſee what will be the play of theſe four ſubſtances, on different ſuppoſitions of the difference of their attractive forces. And firſt, let us ſuppoſe that the differences of the attracting forces of the two alkalis for the two acids are equal, or that each of the alkalis attracts the vitriolic acid more ſtrongly than the muriatic, by an equal ſuperiority of attracting force. In this caſe no exchange can happen: the attractions are exactly balanced.

For, let the attraction of the volatile alkali for the muriatic acid be repreſented by x, and its attraction for the vitriolic acid by x, and ſome other quantity a, or x + a. In like manner, let the attraction of the fixed alkali for theſe two acids be y, and y + b.

The forces which maintain things in their preſent ſtate are x + a and y, the ſum of which is x + y + a.

The forces which tend to change the ſtate of things are x, and y + b, the ſum of which is x + y + b.

It is plain, that if a, the difference of the attractions of the volatile alkali for the two acids, be equal to b, the difference of the attractions of the fixed alkali for them, then x + y + a is equal to x + y + b, and no change can happen.

But if b be greater than a, it is equally plain that x + y + b, the ſum of the changing forces, is greater than x + y + a, the ſum of the maintaining forces. We muſt, therefore, have a change of combinations, and muſt find in the ſolution ſal ammoniac and Glauber's ſalt.

If heat be applied to this mixture, the difference of attracting forces will be ſtill more increaſed, becauſe this, by increaſing the volatility of the volatile alkali, and the muriatic acid, will weaken their connection with their primitive and more fixed baſes.

There are many other ways by which the vitriolic ammoniac can

be decompounded by double elective attraction, but they will be more eaſily underſtood hereafter.

SPECIES VIII.—NITROUS AMMONIAC.

Nitrous Ammoniac, or *Nitrum Semivolatile*, or *Nitrum Flammans*, is fuſible, volatile, and inflammable. The volatile alkali contains inflammable matter, as may be ſeen by trying all the ammoniacal ſalts with melted nitre. It cannot be diſtinctly obſerved in the alkaline cryſtals themſelves, on account of their too great volatility, which makes a violent ebullition, and ſeems to keep the nitre from coming into contact with them. But in all caſes where they can be made to mix in red heat, the inflammation is even brilliant. For example, if the vapour of melted nitre be made to paſs through that of volatile alkali.

This ſalt diſſolves readily in water; and, with the aſſiſtance of heat, very copiouſly. This ſeems to proceed from its fuſibility, for it is not remarkably deliqueſcent. It is decompoſed for the acid by vitriolic acid, and for the volatile alkali by either of the fixed alkalis.

It is ſaid to be contained in the juices of ſome plants, and that it is often found in natural effloreſcences; but when we have occaſion for it, we always prepare it by art. It is neceſſary to obſerve the caution of evaporating the ſolution of it with extremely gentle heat; for, if we go on with a ſtrong one, we may be deceived, and evaporate the whole ſalt before we are aware. *(See Note 33. at the end of the Volume.)*

SPECIES IX.—SAL AMMONIAC.

The laſt of theſe ſalts, the common *ſal ammoniac*, or crude ſal ammoniac, is the beſt known, and the moſt uſeful; and it is a conſi-

derable object of manufacture and commerce. When we examine by trial how it is affected by heat, we find it is one of thoſe bodies which, under the ordinary preſſure of the air, are more eaſily changed into vapour than melted; and therefore, when heated, it totally evaporates in white ſmoke, before it be heated enough to make it melt. It has nearly the ſame attraction for water as common ſalt. It deliquiates in damp air, and diſſolves in about three waters. In ſolution it produces 20 degrees of cold, by Fahrenheit's ſcale. It is eaſily decompounded, ſo as to give either the acid or the alkali pure; but we never take the trouble to ſeparate the acid, which may be got from a much cheaper ſalt. It is only for the ſake of the alkali that we decompound this ſalt: the alkali which it yields being the pureſt volatile alkali.

This decompoſition may be effected by means of either of the fixed alkalis, in their ordinary mild ſtate. A quantity of pearl aſhes, ſalt of tartar, or ſoda, is mixed with about twice its weight of ſal ammoniac, and the mixture is put into a wide-necked retort, and treated in the way of diſtillation.

It is uſual to add water to facilitate the action of the ſalts, and the condenſation of the volatile alkali, which is difficultly condenſed without water. As ſoon as the retort becomes warm, the alkali, and a part of the water, begin to diſtil over together; but the alkali comes in greateſt quantity at firſt, on account of its being the moſt volatile; and the water, which comes with it at firſt, is not ſufficient for diſſolving it: A great part of it, therefore, condenſes, in the form of a ſaline cruſt, on the internal ſurface of the receiver. But afterwards, a larger proportion of water comes over with the reſt of the alkali; ſuch a proportion as is ſufficient for diſſolving very nearly the whole of it. Thus, we have a ſaturated ſolution of volatile alkali in water, which, when prepared in this manner, is called *ſpirit of ſal ammoniac*.

The London chemiſts prepare a dry volatile alkali for ſmelling bottles, which is much more volatile than the *ſal ammoniaci volatile, cum*

creta. They decompoſe the ſalt with fixed alkali, luting up the veſſels very cloſe, with a paſte of flour and glue, and by taking all methods of cooling the receiver. In this way, they get half the weight of the ſal ammoniac.

Origin.——This ſalt is not found native in ſuch quantity as to ſerve any of the uſes to which it is applied. A ſmall appearance of it is ſometimes met with in the neighbourhood of volcanoes, and upon ſtones thrown out of thoſe mountains; alſo in clefts of the ground, or of rocks in the neighbourhood of coal-pits which had been accidentally ſet on fire, and have continued to burn for a long time: But ſuch ſpecimens are extremely rare and trifling. The great quantity of this commodity conſumed in Europe was, for a long time, imported from Egypt, where it was known and manufactured; and ſometimes quantities of it are brought from ſome parts of India. It was late, however, before any diſtinct accounts were publiſhed of the manner in which it is manufactured in Egypt. We were only told that ſoot was the principal article employed; until ſome French gentlemen, who were in Egypt, having given particular attention to it, communicated a detail on this ſubject to the Academy of Sciences. Their account was, that the fuel of the diſtricts in which it is manufactured is the dung of cattle, dried in the ſun; that the ſoot is collected, and without any addition, put into glaſs globular veſſels. Theſe they fix in the vault of a furnace, which has the form of a ſort of oven, built in the open air, and apply fire cautiouſly, &c.

When this account was publiſhed, it was ſuppoſed that the production of ſal ammoniac depended on the particular nature of the ſoot, or of the fuel from which that ſoot is produced; but it has been diſcovered that ſoot of other kinds of fuel, and pit-coal among the reſt, can be made to afford it; and ſeveral manufactories of it are now eſtabliſhed in Europe, which produce it of the beſt quality.

From its being obſerved alſo that the great laboratories of ſal ammoniac alſo prepare Glauber's ſalt, employ much vitriolic acid, and do

not purchaſe the muriatic acid, it is conjectured that the volatile alkali is firſt prepared from the ſoot, then made into vitriolic ammoniac, and this mixed with ſea ſalt, and the mixture ſublimed. In this proceſs, a double exchange muſt take place; and we muſt obtain ſal ammoniac in the receiver, and Glauber's ſalt muſt remain in the retort.

This ſalt is uſed in medicine, and in tinning iron or copper; and in ſome other manufactures.

The ſalts which contain the acetous acid are regenerated tartar and acetous ammoniac.

SPECIES X.—REGENERATED TARTAR.

Regenerated tartar was ſo called, from its component ſalts, the one being the ſalt of tartar, or the vegetable fixed alkali, and the other, one of the vegetable acids. It was ſuppoſed that theſe, when joined together, ſhould produce a ſalt like tartar, in its entire ſtate, or a regenerated tartar. But we now know perfectly well, that the acetous acid which this ſalt contains, is quite different from the acid of tartar; and, accordingly, the compound ſalt is not a regenerated tartar, being very different from tartar. It was formerly called *ſal diureticus*, in the London Pharmacopœia; lately they have changed this name to *kali acetatum*.

This ſalt is always made artificially, by joining pure vegetable alkali with the acetous acid in its pureſt ſtate, or diſtilled vinegar. A large proportion is neceſſary to ſaturate the alkali, on account of its being an extremely weak or diluted acid. A phenomenon occurs in making this mixture, which Dr. Boerhaave mentions, and could not account for. When vinegar is firſt added to the alkali, it diſſolves it without perceptible effervescence; but after more of the acid has been added, ſo that ſome part of the alkaline ſalt is ſaturated, further additions cauſe it to effervesce violently, as in other caſes, when acids and

alkalis are joined together. This phenomenon we ſhall ſoon have an opportunity to explain.

The proceſs for making this ſalt is beſt conducted by putting a quantity of pure fixed alkali into an evaporating bowl, adding diſtilled vinegar, and evaporating the ſalt almoſt dry. Then more vinegar is to be added, and evaporated, until further additions of vinegar do not produce an effervescence; and it is better to add a little too much than too little. Every time the evaporation is performed, it is only the watery parts that exhale; the acid remains combined with the alkali. When full ſaturation is obtained, we may evaporate to dryneſs, which muſt be done with a very mild heat. Thus we have a ſalt of a dark brown colour, and very unctuous and foul. A quantity of fæculent unctuous matter is extricated from the vinegar, when evaporated to dryneſs along with the alkali, though it appeared a limpid fluid before. This unctuous fæculency is ſeparated from the ſalt by covering up the bowl, and gradually increaſing the heat until the ſalt melt, which it does in ſuch a degree of heat as is ſufficient for evaporating the more volatile parts of the oily matter, and ſcorching or burning the reſt, ſo as to convert it into a ſort of charcoal. (N. B. This heat makes poliſhed ſteel yellow, and might be meaſured). By this means the ſalt, which before was brown, becomes of an iron-grey colour; and if it be now diſſolved in water, the burnt matter is rejected or left undiſſolved by the water, and forms a ſediment which may be ſeparated by filtration; and the clear limpid ſolution then affords, by gentle evaporation, a pure and white ſalt.

Many authors, in deſcribing the manner of purifying this ſalt, adviſe to do it by diſſolving in ſpirit of wine, which ſeparates part of its foulneſs from it, and then the ſpirit is to be diſtilled off, and the ſalt rediſſolved in the ſame manner ſeveral times. But, if our only purpoſe is to make it white and pure, this will prove a tedious and imperfect method. The one I have deſcribed is much the beſt.

Dr. Boerhaave, who extols the virtues of this ſalt very highly,

ſeems to think this purification of no great importance, or rather that the ſalt is a better medicine for retaining the oil. He had a prejudice in favour of every thing that contained ſalt and oil blended together, and called all ſuch mixtures ſaponaceous, or ſoapy; but it is certain that this ſalt is greatly improved by the purification I have deſcribed. The unctuous matter thus ſeparated from it, has a bitter taſte and nauſeous flavour, diſagreeable to the ſtomach; and this is a bad quality in any medicine intended for the uſes to which this ſalt is applied.

The properties of this ſalt, as thus prepared, are, that it melts with a very moderate heat, below red, into a tranſparent fluid like oil; and, upon being allowed to cool again, concretes into a maſs compoſed of thin plates, or leaves, cohering together, like the plates of the earthy or ſtony ſubſtances hereafter to be deſcribed under the name of talc or mica; for which reaſon ſome of the elder chemiſts named it very improperly *terra foliata tartari*. When mixed with water, it ſhews a great degree of ſolubility, greatly exceeding every other compound ſalt. It is ſo deliqueſcent that it is not eaſy to keep it dry. It is alſo ſoluble in vinous ſpirits, and is thus purified from other ſalts. When it is to be ſeparated from water, this is done by evaporation to dryneſs with a very gentle heat, ſtirring often in the end. The ſalt concretes in the form of a white ſcum or ſkin, which, being drawn off to a ſide, is quickly renewed, until the whole water be thus evaporated. The ſalt is then a ſpongy maſs, as white as ſnow, which, if the heat be increaſed, melts and forms the foliated maſs which I deſcribed.

When we deſire to decompound this ſalt, we may do it ſo as to have either the alkali or the acid pure.

If we would have the alkali, we need only to put the ſalt into a crucible, and heat it gradually till it becomes red hot. That degree of heat burns and totally deſtroys the acid, diſſipates great part of its principles in the form of fœtid vapours, which are partly watery, partly oily, and partly aërial, and leaves only remaining with the alkali

a quantity of black carbonaceous and other matter, which can be ſeparated without difficulty.

If we deſire the acid, we can detach it by any of the foſſil acids, of which the vitriolic, however, anſwers beſt, and is employed ſometimes for this purpoſe, when an acetous acid of extraordinary ſtrength is deſired. But this purpoſe is better accompliſhed by means of the next ſalt in the column, namely,

SPECIES XI.—THE ALKALI FOSSILE ACETATUM.

This conſiſts of the acetous acid united to the foſſil alkali. Its properties greatly reſemble thoſe of the ſalt now deſcribed, only it melts more readily. It congeals in plates, and is deſtructible by a great heat, like the other. It diſſolves copiouſly in water, and is eaſily ſeparated from the water by cryſtallization, forming very fair cryſtals; whereas the other is cryſtallized with difficulty. This ſalt is better ſuited to the purpoſe of obtaining a ſtrong acid by decompoſition, becauſe it is eaſily kept in the form of a dry powder, not being diſpoſed to deliquiate, except in very damp air. It is therefore much prepared in Germany for this purpoſe, by the name of *ſal vegeto-minerale*. It is not, however, by the ſimple mixture of its component ſalts that it is manufactured, as I am informed by a perſon who long conducted the proceſs, but by double elective attractions, employing Glauber's ſalt, and a ſalt conſiſting of lead combined with the acetous acid. When the ſalt thus prepared is decompounded by vitriolic acid, the acetous acid is detached in a moſt concentrated and active form, equal in acidity to the mineral acids, though weaker in its elective attractions, and poſſeſſing all its native valuable qualities.

SPECIES XII.—ACETOUS AMMONIAC.

Acetous ammoniac, or *vegetable ammoniac*, or *ſpiritus Mindereri* when in a liquid form, ſo called from the author's name, is a compoſition always produced by art. It is very volatile, being little inferior to water in volatility, and at the ſame time of great ſolubility. It cannot be evaporated dry. A great part of the water, however, can be ſeparated by ſlow diſtillation or evaporation; and we thus obtain a ſaline fluid of a thickiſh conſiſtence, like oil or ſyrup, and of a brown colour, which, if the heat be continued, evaporates entirely away without yielding any dry part. It is of a penetrating quality. If we deſire to have a vegetable ammoniac in a dry form, we may have it by a double elective attraction. The ſal vegeto-minerale and common ſal ammoniac, for example, will give it; the muriatic acid uniting with the fixed alkali, while the more volatile acetous acid unites with the volatile alkali. For ſimilar reaſons, we may employ a mixture of vitriolic ammoniac, and the ſalt which I mentioned a little ago, conſiſting of lead combined with the acetous acid. This laſt will more certainly inſure the dry form of the ſalt obtained, but it will differ, I believe, from the ſalt obtained by the other proceſs, containing leſs of the diſtinguiſhing property of the acetous acid; containing it in a ſtate which may be called the acetic, while the other is called the acetous.

The different ways by which it may be decompounded are obvious; but we never have occaſion to decompound it. The only point that deſerves attention with regard to it, is the compounding it properly, or uniting its two ingredients, ſo as to produce it in perfection. This is done by chooſing good diſtilled vinegar, which has been diſtilled carefully, to avoid a burnt taſte and flavour, and a pure volatile alkali, prepared from ſal ammoniac. That which is prepared from animal ſubſtances is unfit. And further, theſe materials muſt be mix-

ed ſo as to be exactly neutralized, the one by the other. Thus, it will be beſt fitted for the purpoſes of medicine for which, it is intended.

Having deſcribed the ſalts in uſe which contain the acetous acid, we proceed to thoſe which contain the acid of tartar.

The compound ſalts, commonly known and in uſe, which contain the acid of tartar, are, 1*ſt*, ſoluble tartar; 2*dly*, common tartar; in both of which this acid is combined with the vegetable alkali in different proportions; and 3*dly*, the Rochelle ſalt, ſal Rupellenſis, or ſal polychreſtus Rupellenſis, in which this acid is combined in part with foſſil alkali.

SPECIES XIII.—TARTAR.

You no doubt remember the appearance and properties of the acid of tartar. It differs from the foſſil acids, and from the acetous, by being capable of evaporation to dryneſs, and a ſort of cryſtallization; which evaporation, however, muſt be performed with a very gentle heat. The dry acid thus obtained, reſembles in appearance brown ſugar, or ſometimes is whiter. And, when we expoſe it to a burning heat, or heat of ignition, it is burnt and deſtroyed in the ſame manner as ſugar, or other vegetable ſubſtances. It is diſſolved eaſily, and in large quantity, by water, forming a ſolution which has the appearance of a ſyrup, but is exceedingly ſour.

The nature of this acid, and of the compounds which it forms with alkalis, was not explained fully and clearly until the ingenious Mr. Scheele of Sweden gave a ſet of deciſive experiments upon this ſubject. It was he who firſt exhibited this acid in its ſeparate ſtate, and who ſhewed how eaſily it is deſtroyed by fire, without needing to be joined to a fixed alkali, as the acetous acid does. Mr. Scheele has alſo ſhewn, that it differs from all the other acids, by a diſpoſition to form with each of the alkalis two different compound ſalts, in conſequence of its being capable of uniting with each of them firmly in two different proportions.

When to the ſolution of an alkali in its ordinary ſtate we add a certain quantity of pure acid of tartar, we have efferveſcence, and we form the compound ſalt, named in the table *Tartarus ſolubilis*, ſimilar in nature to other neutral ſalts, in which neither acid nor alkali can be perceived to predominate. But, after having done this, if we add more of the acid, although there is no more efferveſcence, a certain quantity of this ſuperfluous acid joins itſelf to the neutral ſalt we had formed at firſt, and adheres to it ſtrongly, conſtituting with it another compound ſalt, compoſed of the ſame acid and alkali, but in which the acid predominates. It is no longer a neutral ſalt, but ſhews the qualities of an acid, in mixture with other ſubſtances. It is this acidulous compound of the tartarous acid with the vegetable alkali that is beſt known and moſt uſeful; it is called ſimply *tartar*. It is much uſed in medicine, and in ſome of the arts, and is certainly a very valuable article of the materia medica, on account of its deobſtruent and cooling powers. It is, therefore, worth while to attend to its hiſtory and chemical qualities.

Tartar is not an artificial ſubſtance, but is produced by nature in the juices of ſome vegetables, from which we acquire it. The juices of many vegetable fruits contain compounds of this kind, which reſemble tartar more or leſs. But the ſpecies of this ſaline ſubſtance that is in common uſe in medicine and the arts, is obtained from the juice of the grape, fermented to a wine. The ſharper wines contain the moſt of it, the Rheniſh eſpecially, and, when long kept, depoſit the tartar, which concretes by cryſtallization to the internal ſurface of the caſk, in the form of a coarſe and fæculent ſaline cruſt, called WINE-STONE or ARGAL. The wine itſelf is conſiderably improved by this ſeparation, &c. becoming more ſmooth and palatable, and being therefore more valued.

At the ſame time that tartar thus cryſtallizes, ſome of the colouring matter of the wine ſeparates along with it. Hence we have red tartar and white tartar. Neither of them is employed in medicine, or

much in the arts, in their native impure ſtate. They are commonly made to undergo a refinement, by which the colouring matter of the wine is ſeparated, and the tartar is alſo made purer in other reſpects. This is done by ſolution in boiling water, clarifying the ſolution and cryſtallizing. Thus are produced white cryſtals, or cryſtallized maſſes, called cryſtals of tartar. I muſt refer you for a particular account of this very conſiderable manufacture in the wine countries, to *Macquer's dictionary*, *Rozier's Journal de Phyſique*, *tom.* i. 67; and to the diſſertation of Mr. Fizes, the inventor of the moſt approved French proceſs, as practiſed near Montpellier, publiſhed in the Mem. Acad. des Sciences, 1725.

This refined or cryſtallized tartar is beaten to powder by the apothecaries, and kept in that form for uſe; and in that ſtate is often called for and preſcribed under the name of cream of tartar. The reaſon of this denomination is, that it was long procured by evaporation to a pellicle, which was ſkimmed off like cream, and is renewed almoſt as faſt as it is removed.

Let us now examine the chemical qualities of this ſubſtance.

When expoſed to a burning heat, the acid, as you might expect, is conſumed and deſtroyed; and we find the alkali, with which it was combined, in the coal or cinder that remains.

We had occaſion to remark formerly, that the alkali of tartar is remarkably pure. The burning of this ſalt is therefore an operation frequently performed to obtain it; and the alkali thus obtained was formerly called ſalt of tartar. The preparation of it is preſcribed in all the pharmacopœias.

It is a very ſimple proceſs, and may be performed either by burning the tartar in a crucible or an iron pot, or by laying it among burning coals in a moderate fire. A ſtrong fire would be unfit for this purpoſe, as being capable of melting or even evaporating the alkali. It is uſual to take the crude tartar, and to wrap it up in wet paper before it be put into the fire; it then ſinges and coheres like feathers or hair,

and forms a ſpongy cinder or coal, which muſt be left in the fire until it ceaſes to give ſmoke and flame; after which we ſhall find it eaſy to extract the alkali from the earthy and coaly matter of the cinder by water.

When we next examine the nature of tartar, by mixing it with other ſubſtances, we find, firſt, that it has but little ſolubility in water, eſpecially if the water be cold. It then not only diſſolves ſlowly and difficultly, but requires a very great quantity of the water. To make the tartar diſſolve more readily, we muſt apply the water boiling hot, or boil them together. Even of the boiling water, eighteen or twenty times its weight are required; and when the ſolution becomes cold again, the tartar ſeparates by cryſtallization almoſt entirely.

In mixture with other things, tartar ſhews the qualities of an acid, in conſequence of the predominant or ſuperfluous acid which it contains. It reddens the vegetable colours, and it effervesces with alkalis. The alkali unites with the ſuperfluous part of the acid, and changes the tartar into a perfect neutral ſalt. With the vegetable alkali it forms *ſoluble tartar*, or *tartarized tartar*. With the foſſil alkali it forms Rochelle ſalt, ſal Rupellenſis, alſo called ſel polychreſte of Seignette, a mixed neutral, containing two alkalis. With the volatile alkali it forms another mixed neutral; alſo with two alkalis, which has not been applied to any uſe.

All theſe neutralized tartars are much more ſoluble in water than tartar itſelf; and hence the name of ſoluble tartar which they had in common. They can be decompounded again, ſo as to reſtore the tartar to its former ſtate, by adding any foſſil acid, or even the acetous. The added acid joins with the *neutralizing* part of the alkali, and thus reſtores the tartar to its former condition. This has been known to happen in preſcriptions, by the acid of tamarinds. But it is remarkable that, after the other acids have thus ſeparated the neutralizing part of the alkali, they have little power over the reſt, and act on it but ſlowly and imperfectly. The abundance of the

acid of tartar defends it from their action; a caſe which occurs not unfrequently in chemical combinations.

You now underſtand what ſort of neutral compounds are formed, when we ſaturate with the different alkalis the ſuperfluous acid contained in tartar or cremor tartari, and in what manner this acidulous compound ſalt may be decompounded.

In the firſt place, if we deſire the alkali, the common way, as I obſerved before, is to deſtroy and conſume the acid by fire. The alkali remains unchanged.

The manner of obtaining the pure acid part of tartar is not ſo obvious, becauſe, as I have juſt now obſerved, the acids which may be employed for diſengaging it, after having combined with what may be called the neutralizing part of the alkali, act but feebly on the reſt, and detach it ſtill united with part of the tartarous acid; and we obtain an acidulous ſalt which reſembles the cream of tartar ſo much, that it was concluded to be the ſame, and chemiſts inquired no further about it. The decompoſition of the tartarine neutrals ſeemed as good as that of nitre or common ſalt.

But Mr. Scheele was induced, by many reaſons, to think that this decompoſition was very imperfect.

When chalk, or ſome other earthy ſubſtances of the ſame nature, which we ſhall ſoon deſcribe, is thrown into a hot ſolution of tartar, it produces an effervеſcence, which is renewed by more chalk, until a certain quantity has been added, after which no more effervеſcence can be produced. The liquor being then allowed to cool and ſettle, depoſits a plentiful white ſediment.

Mr. Scheele, examining the clear fluid from which this ſediment had ſubſided, found that it contained a tartarus ſolubilis, or neutralized tartar. He therefore concluded that the chalk had deprived the tartar of a *ſuperfluous* acid which it contained, and had left no more combined with the alkali than what was neceſſary to conſtitute a ſalt perfectly neutral.

This idea induced him to examine the ſediment, and in it he actually found a part of the acid of the tartar, combined with the chalk. They form together a compound, which has very little, or ſcarcely any ſolubility in water. The moment it is formed, therefore, it concretes into a quantity of ſmall cryſtals or concretions, like fine ſand, much exceeding the chalk employed. And as by this want of ſolubility it reſembles a compound of chalk and vitriolic acid, to which the chemiſts and natural hiſtorians formerly gave the name of *ſelenites*, he called the new compound which he had diſcovered *ſelenites tartareus*.

He next diſcovered that the acid of tartar may be extracted from this compound, by the action of the vitriolic acid, which has a ſtronger attraction for the chalk than the acid of tartar has.

When I ſay this, ſome of you may perhaps imagine that it may be extracted by an operation ſimilar to that by which the nitrous and muriatic acid are obtained from nitre and common ſalt; I mean by adding the vitriolic acid, and then diſtilling the mixture, that the acid of tartar may riſe in diſtillation. But if any perſon imagines ſuch a proceſs could ſucceed, he has forgot the nature of the acid of tartar. The acid of tartar cannot be diſtilled: It cannot riſe with water in diſtillation, but remains behind, while the water is diſtilled or evaporated from it. And when we attempt to convert it into vapour after this, it is deſtroyed by the heat, in the ſame manner as wood or ſugar, or the like vegetable ſubſtances.

But diſtillation in this caſe is not neceſſary. The ſeparation is practicable, in conſequence of the difference of ſolubility. The acid of tartar is exceedingly ſoluble in water. The vitriolic ſelenite, from which it is to be ſeparated, has very little ſolubility. To obtain the acid of tartar by itſelf, we muſt therefore proceed in this manner: Firſt, waſh away all the neutral ſalt from the *ſelenites tartareus*; and, ſecondly, add a proper quantity of vitriolic acid, diluted with four times its bulk of water. This acid immediately unites with the chalk,

and ſeparates from it the acid of tartar. The vitriolic acid, in uniting with the chalk, forms with it a compound that is almoſt inſoluble in cold water, and forms a white ſediment in this proceſs. The acid of tartar, being, in its ſeparate ſtate, exceedingly ſoluble, is found diſſolved in the clear part of the fluid, after ſubſidence of the ſediment, and can be poured off, or it may be filtrated; and then we can evaporate it with a gentle heat to dryneſs; or it may be cryſtallized. As firſt obtained, however, it is not perfectly pure; a ſmall portion of the vitriolic ſelenite adheres to it; but it can be purified by diſſolving it again in a ſmall quantity of water, which will leave the ſelenite undiſſolved.

We have now, therefore, a clear view of the nature of tartar, and of the acid which it contains; which is truly ſingular in many reſpects: For, beſides being capable of uniting firmly with alkalis, ſo as to form either neutral or acidulous compound ſalts, it adheres to the alkali, with different degrees of force, according to the quantity of it. The other acids, the foſſil for example, or the acetous, when applied to a neutral or ſoluble tartar, very readily unite with, and ſeparate from it part of the alkali; and thus reduce it to the ſtate of tartar again, or *cremor tartari*. But, after they have done this, they have much leſs power over the remaining alkali which it contains. The abundance of acid of tartar with which it is combined, defends it, or adheres to it more ſtrongly; and probably repels the particles of the other acid. It does not, however, prevent entirely the action of the foſſil acids; for if theſe are applied in large quantity to the *cremor tartari*, they certainly produce a decompoſition of it, by uniting with the alkali.

But the difficulty or impracticability of decompounding it, by moderate quantities of other acids, concealed the nature of this compound ſalt for a long time.

It is alſo remarkable that, although the vitriolic acid detaches the

acid of tartar from the alkali, the pure acid of tartar will partly decompoſe the vitriolated tartar, nitre, and digeſtive ſalt.

To finiſh this ſubject, I ſhall only further notice, that as ſoon as I learned this proceſs of Scheele's, I diſcovered that the acid of tartar may be extracted more completely by a little variation of Mr. Scheele's proceſs, viz. by uſing quicklime in place of chalk. But of this more hereafter.

I muſt alſo obſerve, before quitting this ſubject of tartar, that, beſides this agreeable acid obtained from the fermented juice of the grape, there is a variety of acidulous ſalts obtainable from other vegetables, in their native ſtate, which ſeem to be of the ſame kind, or at leaſt very analogous to the tartar already deſcribed. I am convinced, from repeated experiments made by myſelf, from the very beginning of my chemical ſtudies, that all the native vegetable acids, whether thoſe which are unfolded during the maturation of their fruits, or thoſe contained in the native juices of the plants, as we find it in the whole tribes of rumices and acetoſæ, reſemble the tartar of wine in this chief circumſtance, that they contain a fixed alkali in their compoſition, ſuper-ſaturated with an acid. All theſe dry ſalts, and ſour juices, give the ſame alkali by incineration. Their differences ſeem to proceed entirely from ſmall variations in the acid, conſiſting either in a different proportion of the parts truly ſaline, or more generally from mucilaginous or reſinous matter. Theſe varieties are the characteriſtics of the different kinds, the chief diſtinctions of which are the degrees of ſolubility of the compound ſalts, formed by their union with the alkali. All of them are equally deſtructible by fire, and incapable of being volatilized. I am therefore diſpoſed to think that the real acid is the ſame in all, and that their diſtinctions ariſe from ingredients not ſaline. One of theſe forms is the tartar of wine. I would ſay that another forms the tartar of ſorrel, &c. In ſhort, I would call them all tartars, were it not that it is a barbarous appellation. I am further of opinion that they are all ultimately reſolvable

into acetous acid. I think that their denominations have been multiplied without reaſon: Malic, citric, oxalic, &c. anſwer no purpoſe.

SPECIES XIV.—BORAX.

We come now to the laſt compound ſalt in the table, Borax. This ſalt was long known and employed in the arts, before the nature and compoſition of it was underſtood. The chemiſts found that it poſſeſſed ſome alkaline qualities, as taſte, and changing vegetable colours to green. They were, therefore, inclined to conſider it as a particular ſpecies of alkali. But this idea of it was not readily adopted, on account of its not effervеſcing in the leaſt with acids. At laſt, however, its nature was thoroughly explained by the experiments of ſome of the French academicians, which ſhewed it to be a compounded ſalt, containing that peculiar acid, the ſedative ſalt, joined with the foſſil alkali. And hence the alkaline qualities of it appear now to be a conſequence of the weakneſs of the acid with which the alkali is combined in this ſalt.

All the Borax uſed in Europe has hitherto come from the Eaſt Indies, in the form of cryſtals, which cohere together by means of unctuous matter; and the whole is called *Tincal*, or *Tincal paſte*. This Borax is refined in Europe by a proceſs which is ſtill kept ſecret; and was for a long time practiſed only in Holland, though ſurely there can be no great myſtery in it. The refined borax is ſold to goldſmiths, and other artiſts, who make uſe of it in the form of large cryſtals, ſomewhat reſembling alum.

The effects of heat ſhew that there is much water in the compoſition of theſe cryſtals. They readily undergo the watery fuſion, and then ſwell up into a very ſpongy bliſtered maſs. When the water is at laſt diſſipated, they become a clear glaſſy ſalt, having all the tough

ductility of glaſs when hot, and, when cold, ſplitting and breaking preciſely like a piece of glaſs. But it is ſtill a perfect ſalt, diſſolving completely in a large portion of water, and cryſtallizing as before.

The compound nature of this ſalt is eaſily demonſtrated, and the two ingredients of it ſeparated from one another. The eaſieſt and ſhorteſt method to ſeparate the ſedative ſalt is to diſſolve the borax in a ſmall quantity of hot water, and to add a proper quantity of any of the foſſil acids, which immediately unites with the alkali, and then allow the ſolution to cool. The ſedative ſalt ſeparates by cryſtallization. This is the only example of the decompoſition of a ſalt in this particular manner. I may add, that the alkali is obtained in its pureſt ſtate, when we employ the nitrous acid for the decompoſition, and then deflagrate the cubic nitre.

From the proceſs, it is plain that foſſil acids have a much ſtronger attraction for the alkali than the ſedative ſalt has. There is, however, a fact, which, when I mention it, will at firſt appear inconſiſtent with this ſuperiority of attraction. The fact I mean is this, viz. the decompoſition of nitre and common ſalt by ſedative ſalt. It is not, however, inconſiſtent with what happens in the proceſs, but is an example of an elective attraction in conſequence of heat. The volatility of the nitric or muriatic acid, when the compound is urged by a great heat, diminiſh its attraction for the alkali, while the attraction of the ſedative ſalt ſuffers little or no diminution. The ſedative ſalt has not, however, the power to ſeparate the vitriolic acid in the ſame manner. If we mix ſome cryſtallized ſedative ſalt with Glauber's ſalt, or vitriolated tartar, and expoſe the mixture to heat, nothing happens but the ſublimation of a ſmall part of the ſedative ſalt. Hence the proceſs for ſedative ſalt by ſublimation. (Vide Experiments on the Sedative Salt by Bourdelin, Mem. de l'Acad. 1753 and 1755.)

Borax is uſed in melting gold, ſilver, and ſome other metals, and in ſoldering them, and in the compoſition of very fine glaſſes, or paſtes as they are called. It is alſo exceedingly uſeful in making experiments with the blow-pipe.

With reſpect to the origin of this ſalt, I ſaid already that hitherto all that we have had in Europe has been imported from the Eaſt Indies, under the name of *tincal*, or *tincal paſte*. The account given of its origin in that part of the world is, that it is produced from the waters of certain mineral ſprings, which, being evaporated by the heat of the ſun, afforded this ſalt. (See ſeveral particulars relative to the production of borax, in a letter from Mr. Blane at Lucknow, Phil. Tranſ. vol. 77. Part 2d.) Nothing like it, however, had been obſerved in any of the mineral waters of Europe, until a few years paſt, that, in Tuſcany, the Duke's apothecary having examined certain hot ſprings in a volcanic part of his Highneſs's dominions, found various ſalts and other mineral ſubſtances, which had been depoſited by the waters; and, among theſe ſaline ſubſtances, he found conſiderable quantities of ſedative ſalt, from which we are told borax has ſince been manufactured. This I had occaſion to mention when ſpeaking of the ſedative ſalt.

Borax and tartar, or *cremor tartari*, afford a rare example of two compound ſalts uniting together, and acting ſomehow on one another without the decompoſition of either. This appears when we mix ſixteen ounces of cremor tartari with two ounces of borax. We ſhould expect ſomething like this, for tartar is ſenſibly acid, and borax is ſenſibly alkaline. Theſe ſalts enter into ſome ſort of union, in conſequence of which the cremor tartari becomes very ſoluble in water, although it ſtill retains its acidity as before. No part of its acid appears to be taken from it by the borax, nor is the borax itſelf decompounded; for no part of the ſedative ſalt ſeparates by cryſtallization. This may, therefore, be conſidered as a caſe in which chemical attraction has ſufficient power to produce one effect, viz. a diminution of coheſive attraction, but not enough of power to produce the full effect, which is produced in the moſt of ſuch caſes, I mean exchange or decompoſition. The German phyſicians, I am informed, make uſe of

this power of borax to increaſe the ſolubility of cremor tartari in pharmacy and preſcription.

SYNONYMOUS DENOMINATIONS OF THE SALTS.

Many of theſe ſalts, which I have deſcribed as reſulting from the combinations of the alkalis and the acids, were ſubſtances never ſeen, or never obſerved before, and therefore were things without a name. Yet they muſt be denominated. Their names were given in conſequence of ſome circumſtance accompanying their formation. Sometimes the ingredients from the admixture of which they reſulted; ſuch as *vitriolated tartar*, *tartarized tartar*—ſometimes expreſſing what was thought to be their nature, *regenerated tartar*—ſometimes from their medical effects, real or ſuppoſed, *digeſtive ſalts*—ſometimes their diſcoverer, or the place of their firſt appearance, *Glauber's ſalt*, *Epſom ſalt*—ſometimes a mere whim, *ſal de duobus*, *arcanum duplicatum*, &c.

Moreover, as what occurred to one perſon in the courſe of his chemical operations may as readily occur to another, and, particularly, as the ſame ultimate compoſition may reſult from very different proceſſes, it muſt have often happened, that a ſubſtance denominated in one way by one diſcoverer, got a different denomination in the hands of another chemiſt; and it may often be ſome time before theſe two names are found to belong to one and the ſame ſubſtance, and it may even require a good deal of examination to prove the identity.

From ſuch cauſes as theſe, it has happened that almoſt every ſalt that I have mentioned has been known by ſeveral different names. Not only the accidental circumſtances now noticed, but alſo the languages of different nations and of different claſſes of people, have augmented this liſt of ſynonymes. Novel objects imported from other countries, will retain, in a different language, the name which they

had at home. Men of ſcience will ſometimes allow a thing to retain the name which it got among the workmen in whoſe daily occupations it firſt occurred. Such names, having no other ſignificancy, will be conſidered as pure appellations or *proper names*.

In reading the writings of the chemiſts, we ſhall be in a continual puzzle, and may often be obliged to go back through a train of diſcuſſion, before we are certain what ſubſtance it is whoſe effects and properties are engaging our attention.

Therefore, I thought that it would be uſeful to collect, under each of the denominations that we have been conſidering, all the ſynonymous appellations by which you will find them mentioned in the chemical writings. This will appear a diſagreeable farrago at firſt ſight, and many names will appear ridiculous or barbarous; but our future diſquiſitions will frequently ſhew ſome reaſonable account for their getting ſuch names.

For a long ſeries of years, while chemiſtry was only a collection of proceſſes in the various chemical arts, theſe denominations being accidental, and in the language of perſons of no general cultivation, were altogether anomalous, and frequently ſuggeſted very falſe notions of the objects. As chemiſtry began to aſſume a ſcientific form, and its profeſſors were perſons of more extenſive knowledge, theſe impropriieties were obſerved, and great inconvenience was frequently found in employing them. Attempts were therefore made by ſome chemiſts to reform the nomenclature, by reducing it to ſome rule, founded on a principle pervading the whole, and equally applicable to any new diſcoveries which may be made from time to time.

The firſt of theſe attempts, of any note, was made by the late Profeſſor Bergmann of Sweden, whoſe eſſays and treatiſes on different ſubjects in chemiſtry contain a rich fund of accurate experiments and uſeful facts. His plan was this—

In the firſt place, he named the alkalis and acids as they are named in the table before you. And the compound ſalts were expreſſed by

the name of the alkali which they contained, with an adjective formed from the name of the acid; thus,

Vitriolated Tartar	is	Alkali vegetabile vitriolatum.
Glauber's Salt		Alkali foſſile vitriolatum.
Vitriolic Ammoniac		Alkali volatile vitriolatum.
Nitre		Alkali vegetabile nitratum.
Cubic Nitre		Alkali foſſile nitratum.
Nitrous Ammoniac		Alkali volatile nitratum.
Digeſtive Salt		Alkali vegetabile ſalitum.
Common Salt		Alkali foſſile ſalitum.
Common Sal Ammoniac		Alkali volatile ſalitum.
Regenerated Tartar or Sal Diureticus		Alkali vegetabile acetatum.
— — —		Alkali foſſile acetatum.
Vegetable Ammoniac		Alkali volatile acetatum.
Soluble Tartar		Alkali vegetabile tartariſatum.
Rochelle Salt		Alkali foſſile tartariſatum.
— — —		Alkali volatile tartariſatum.
Borax		Alkali foſſile boraxatum.

The plan of this nomenclature was ſuggeſted by ſome of the old names, in which ſomething like this was attempted, though in a bungling and partial manner; and it has the advantage of being applicable to a multitude of other compounds, which contain acids combined with earthy or metallic bodies. It was, therefore, well received, and many of theſe new names were adopted in the laſt editions of the Edinburgh Pharmacopœia, and in many others abroad.

It has, however, been objected that theſe names are too long; and, as it is neceſſary in expreſſing the particular ſtate of ſome of theſe ſalts, to add another adjective, the name becomes, by its length, cumberſome and diſagreeable.

Notwithſtanding this objection, the advantages of a regular and decent nomenclature were ſo ſenſible, that this method became very prevalent, and would, in all probability, have ſoon obtained the ac-

quieſcence of all the philoſophical chemiſts, had not the ſcience itſelf ſuſtained, at this very time, a great and almoſt total revolution.

A number of moſt important diſcoveries were made, about this time, by the chemiſts of Britain, France, and Sweden, relating to the conſtituent parts or principles of natural ſubſtances. New objects have been diſcovered, which required new names; and ſome of the ſubſtances already known, which had formerly been conſidered as ſimple, and perhaps elementary, were now found to be compounded; while others, formerly held as compounds, were now found to be more ſimple than the ſubſtances of which they had been thought to be compounded. This being the caſe, it is plain that, while the *method* propoſed by Bergmann was ſtill advantageous, the *conſtruction* of the nomenclature muſt neceſſarily be changed, that it may be conſiſtent with the knowledge we have acquired.

By a moſt ſagacious and careful conſideration of thoſe diſcoveries, Mr. Lavoiſier formed an opinion concerning the compoſition of bodies, and the principles by which this compoſition was effected, which comprehended the whole of chemiſtry, in ſuch a way as to point out a ſeries of compoſitions in many degrees of ſubordination. This muſt evidently regulate the conſtruction of a nomenclature, on Mr. Bergmann's plan. This philoſopher, therefore, aſſociated ſeveral other eminent chemiſts in his labours, and the ſtudy ſoon acquired, in their hands, very great improvements. Aſſembled in Paris in 1787, and, confident of the ſuperiority of what they called *French Chemiſtry* over all former doctrines and theories, they adopted a plan by which they hoped to give it univerſal currency and authority. They hoped to effect this by means of a nomenclature, ſo adapted to their ſyſtem, that the very denominations of the different objects ſhould imply the doctrines of their theories; ſo that, by uſing this language, it ſhould ſcarcely be poſſible to think on chemical ſubjects, in a way different from their theories. They framed the language ſo that the compoſition of the moſt complex ſubſtance

ſhould be expreſſed by two or three words, modified by proper terminations, and ſo that the preciſe place and degree of ſubordination ſhould be clearly and compendiouſly indicated.

With this view Meſſrs. Lavoiſier, De Morveau, Berthollet, and Fourcroy, publiſhed the *Method of Chemical Nomenclature*, to which they annexed a correſponding ſyſtem of chemical characters, compoſed (I believe) by Meſſrs. Adet and Haſſenfratz. This was publiſhed at Paris in 1787, and is adopted by Mr. Lavoiſier in his Elements of Chemiſtry, as alſo by Fourcroy and Chaptal, and, indeed, by almoſt all the French chemiſts.

I cannot here explain to you the whole of this new nomenclature. Many of the facts on which it is founded have not yet been made known to you. But when we ſhall have advanced further in this courſe, you will eaſily underſtand it, by conſulting the books I juſt now mentioned. In Mr. Lavoiſier's Elements, the new ſyſtem and new language are ſtated with the greateſt preciſion and clearneſs imaginable. (Dr. Pearſon of London has lately publiſhed a treatiſe to recommend the new language). At preſent we can only give our attention to the names propoſed for the ſalts in that ſyſtem. This will be fully ſufficient for enabling you to ſee and underſtand the general rules, by which the denominations and the whole phraſeology are offered and conducted.

One of the general rules which they thought it proper to obſerve in contriving their new nomenclature was, to give names ending with *ſimilar* ſounds, to all the things that are analogous in the *mode* of compoſition, and names ending with *different* ſounds to things which differ in this reſpect: For example,

Firſt, the acids, when they are in their moſt perfect, or moſt powerful, or moſt acid ſtate, are,

Acide ſulphurique,—*acidum ſulphuricum*,—ſulphuric acid.

Acide nitrique,—*acidum nitricum*,—nitric acid.

Acide muriatique,—*acidum muriaticum*,—muriatic acid.

Acidum aceticum,—*acide acetique*,—acetic acid.
Acide tartarique,—*acidum tartaricum*,—tartaric acid.
Acide boracique,—*acidum boracicum*,—boracic acid.

But ſome of theſe acids appear on ſome occaſions in a leſs perfect, or leſs acid, or leſs powerful ſtate, by reaſon of a redundancy of their diſtinguiſhing ingredients. For example, the vitriolic acid, when it is in its volatile and ſuffocating form, is ſo weak an acid in that ſtate, that although it can be joined to alkalis, and form compound ſalts, ſuch ſalts can be decompoſed by the weakeſt acids, ſuch as vinegar, or the acid of tamarinds, or the acid of tartar.

The nitrous acid is another example. In its moſt volatile and deep-coloured ſtate, it has but a weak attraction for alkalis and for water, and other ſubſtances.

To diſtinguiſh this ſtate of the acid, the French chemiſts, inſtead of the termination *-ique*, derived from the Latin *-icus -a -um*, which is the form of a generic epithet, employ the termination *-eux*, derived from the Latin *-oſus -a -um*, which is the form of an epithet expreſſing a redundancy of the ſubject or quality. Thus,

Acide ſulphureux,—*acidum ſulphuroſum*,—ſulphurous acid.
Acide nitreux,—*acidum nitroſum*,—nitrous acid.

Their names of the alkalis are,

FOR THE VEGETABLE ALKALI,

Potaſſe,—*potaſſa*,—potaſh.

FOR THE FOSSIL ALKALI,

Soude,—*ſoda*,—ſoda.

FOR THE VOLATILE ALKALI,

Ammoniaque,—*ammoniacum*,—ammoniac.

In naming the compound ſalts, they uſe two words, the firſt ſuggeſting the acid, and the ſecond the alkali; or, in general, the *acidifiable baſe*.

All the compound ſalts, and other compounds that contain the ſulphuric acid in its more perfect ſtate, are called *Sulphats; Sulphas* in Latin.

All thoſe that contain the nitric acid in its moſt perfect or powerful ſtate, are called *Nitrats; Nitras.*

All thoſe that contain the muriatic acid are called *Muriats; Murias.*

All thoſe that contain the acetous in its moſt perfect or powerful ſtate, are called *Acetats; acetas.*

In conformity to this rule, we have the

Sulphat of potaſh,—of ſoda,—of ammoniac.
Nitrate of potaſh,—of ſoda,—of ammoniac.
Muriate of potaſh, &c.

Theſe compounds are diſtinguiſhed from thoſe in which the acids are redundant in their diſtinguiſhing ingredient, by giving to the latter the termination *-ite*, from the Latin *-is*, or *-ites:* Thus,

Sulphite,—*Sulphis*, or *Sulphites*, of potaſh, &c.
Nitrite,—*Nitris*, or *Nitrites*, of potaſh, &c.
Acetite,—*Acetis*, or *Acetites* * of potaſh, &c.

The acid of tartar is alſo conſidered as an imperfect acid; and therefore they call the compounds formed by it *Tartrites; Tartris* in Latin.

The compounds that contain the ſedative ſalt are called *Borats; Boras.*

Theſe names have evidently been contrived to ſuit the genius of the French language, in the firſt place, and then have been transferred into the Latin words; or the words which were meant to be uſed in the Latin language have been coined from the French ones. When

* We ſhall ſee afterwards that muriates are not properly named. EDITOR.

changes are thus made in the names of things which are familiar to us, I believe moſt people find them diſguſting at the firſt, on account of the ſhock and derangement which they give to the habits we had formed before. Theſe latiniſed French words appeared to me at firſt very harſh and diſagreeable. This, it muſt be confeſſed, cannot be avoided, in making an attempt of this kind, and muſt therefore be overlooked.

When this rage for reformation and innovation was going round, it was natural for every perſon to think a little on the ſubject, and conſider of what he would propoſe, were it required of him to give his opinion: And, if you will give me leave, I ſhall ſtate to you what has occurred to myſelf.

When I give my plan for naming the ſalts, you will think perhaps that I have as little mercy for languages as other people; but I remarked before, that an attempt to coin new names, eſpecially in our ſcience, cannot be made without tranſgreſſions of this kind.

The following ſmall table of the neutral ſalts will give you a ſufficient notion of my plan:

DR. BLACK'S TABLE OF NEUTRAL SALTS.

	Acidum Sulphuricum.	Acidum Nitricum.	Acidum Muriaticum.	Acetum.	Acidum Œnolithicum.	Acidum Boracicum.
Lixiva.	Lixiva Sulphurica.	Lixiva Nitrata, or Nitrica ſive Nitrum.	Lixiva Muriatica.	Lixiva Acetoſa.	Lixiva Œnolithica, et Œnolithus, ſive Lix. Œn. Acidula.	Lixiva Boracica.
Trona.	Trona Sulphurica.	Trona Nitrata, or Nitrica.	Trona Muriatica ſive Muria.	Trona Acetoſa.	Trona Œnolithica. Trona Œn. Acidula. Œnolithus Tronatus.	Trona Boracica, ſive Borax.
Ammonia.	Ammonia Sulphurica.	Ammonia Nitrata, or Nitrica.	Ammonia Muriatica ſive Sal Ammoniacus.	Ammonia Acetoſa.	Ammonia Œnolithica Am. Œn. Acidula. Œn. Ammoniatus.	Ammonia Boracica.

The volatile vitriolic acid will be acidum ſulphureum; and the compounds of it with alkalis, ſulphurea. And ſulphur itſelf, when combined with alkalis, may be ſulphur lixiviatum, ſulphur tronatum, ſulphur ammoniatum, or ammoniacum. Or lixiva ſulphurata, trona ſulphurata, ammonia ſulphurata; which is perhaps better, on account of the more extenſive application of the epithet; thus argentum ſulphuratum, calx ſulphurata, ferrum ſulphuratum, &c.

To expreſs the compound ſalts formed by the other acids, I may proceed as follows:—lixiva ſuccinata, lixiva formicata, fluorata, citrata, phoſphorata, &c. &c. *(See Note 34. at the end of the Volume.)*

The many different names which have been given to the ſame ſalt by its occaſional diſcoverers, are a great inconvenience, and cauſe much obſcurity in reading their writings. To remove this in ſome meaſure, I have collected the moſt uſual ſynonymes, and arranged them under the leading names which I have juſt now been explaining to you. With very little trouble you may arrange them, either according to Bergmann's or the French method. You will find the liſt very uſeful.

SALIUM ALKALINORUM SYNONIMA.

1. LIXIVA.
2. Alkali fixum vegetabile.
3. Kali. Pharm. Lond.
4. Potaſſa. Gallis.
5. Sal Tartari.
6. Sal Abſynthii.
7. Cineres clavellati. Nitrum fixatum.
8. Oleum Tartari.
9. Lixivium Tartari.
10. Aqua Kali. Lond.

1. TRONA.
2. Alkali fixum foſſile.
3. Soda. Pharm. Edin.
4. Natron. Lond.
5. Soda. Gallis.

1. AMMONIA.
2. Alkali volatile. Edin.
3. Ammonia. Lond.
4. Ammoniaca. Gallis.
5. Sal volatile ammoniaci.
6. Sal cornu cervi.
7. Sal Urinæ.

Aqua dilutum.

8. Spiritus ſalis ammoniaci.
9. Aqua ammoniæ.
10. Spiritus cornu cervi.
11. Spiritus Urinæ.

SALIUM ACIDORUM SYNONIMA.

1. ACIDUM SULPHURICUM. Sulphuroſum.
2. Acidum vitriolicum. Pharm. Lond. et Edin.
3. Oleum vitrioli, vel acidum, vel ſpiritus vitrioli.
4. Acidum vel ſpiritus ſulphuris per campanam.
5. Spiritus aluminis.

6. Acidum chalcanthis.
7. ——— primigenium.
8. ——— catholicum.
9. ——— univerſale.
10. ——— aëreum.
11. ——— vagum foſſile.

1. Acidum Nitricum. Nitroſum.
2. Acidum nitroſum. Pharm. Lond. et Edin.
3. Spiritus nitri.
4. Spiritus nitri Glauberi.
5. Aquafortis.

1. Acidum Muriaticum.
2. Spiritus ſalis marini.
3. Spiritus ſalis Glauberi.
— Acidum muriaticum oxygenatum.

1. Acetum.
2. Acedum acetoſum.
3. Acetum deſtillatum.
4. Spiritus aceti.
— Acidum aceticum.

1. Acidum Œnolithicum.
2. Acidum tartari vel tartaroſum.

1. Acidum Boracicum. Boracinum.
2. Sal ſedativus.
3. Sal ſedativus vel narcoticus Hombergii.

SALIUM COMPOSITORUM SYNONIMA.

1. Lixiva Sulphurica.
2. Alkali vegetabile fixum vitriolatum. Edin.
3. Kali vitriolatum. Lond.
4. Sulphas Potaſſæ.
5. Sal polychreſtus.
6. Nitrum vitriolatum.
7. Sal enixum.
8. Sal e duobus.

1. LIXIVA NITRATA. Nitrosa.
2. Nitrum.
3. Nitras potassæ. Gallis.
4. Sal petræ.

1. LIXIVA MURIATICA. Muriata.
   Lixiva muriata oxygenata.
2. Murias potassæ. Gallis.
3. Sal digestivus vel febrifugus Sylvii.
4. Sal marinus regeneratus.
5. Spiritus salis marini coagulatus.

1. LIXIVA ACETATA.
2. Alkali fixum vegetabile acetatum.
3. Kali acetatum.
4. Acetas potassæ—Acetis. Gallis.
5. Tartarus regeneratus.
6. Sal diureticus.
7. Terra foliata tartari.

1. LIXIVA ŒNOLITHICA.
2. Alkali fixum vegetabile tartarisatum.
3. Kali tartarisatum.
4. Tartris potassæ. Gallis.
5. Sal vegetabile.
6. Tartarus solubilis.
7. Tartarus tartarisatus.

1. ŒNOLITHUS.
2. Tartarus. Ed. & Lond.
3. Tartarus acidula potassæ. Gallis.

1. TRONA SULPHURICA.
2. Soda vitriolata. Ed.
3. Natrum vitriolatum. Lond.
4. Alkali fixum fossile vitriolatum.
5. Sal Glauberi.
6. Sal mirabile Glauberi.
7. Sulphas sodæ. Gallis.

1. TRONA NITRATA.
2. Nitras ſodæ. Gallis.
3. Nitrum cubicum.
4. Nitrum quadrangulare.

1. TRONA MURIATA.—Muriatica.
2. Muria. Ed.
3. Sal marinus. Ed.
4. Sal muriaticus.
5. Natrum muriatum. Lond.
6. Murias ſodæ. Gallis.
7. Sal communis.
8. Sal gemmæ.

1. TRONA ACETOSA.
2. Acetis ſodæ. Gallis.
3. Sel vegeto-mineral.

1. TRONA ŒNOLITHICA.
2. Tartris ſodæ. Gallis.

1. ŒNOLITHUS TRONATUS.
2. Soda tartariſata. Ed.
3. Natron tartariſatum. Lond.
4. Sal rupellenſis.
5. Sal polychreſtus rupellenſis.

1. TRONA BORACINATA.—Boracica.
2. Borax.
3. Boras ſodæ. Gallis.

1. AMMONIA SULPHURICA.
2. Sal ammoniacus vitriolicus.
3. Sulphas ammoniacalis. Gallis.
4. Ammoniacus ſecretus Glauberi.

1. AMMONIA NITRATA.
2. Nitras ammoniaci. Gallis.
3. Nitrum ſemivolatile.
4. Nitrum flammans.

1. AMMONIA MURIATA. Muriatica.
2. Sal ammoniacus. Ed.
3. Ammonia muriata. Lond.
4. Murias ammoniacalis. Gallis.

1. AMMONIA ACETOSA.
2. Spiritus Mindhereri. Edin.
3. Aqua ammoniæ acetata. Lond.
4. Acetis ammoniacalis. Gallis.

1. AMMONIA ŒNOLITHICA.
2. Tartris ammoniacalis. Gallis.

*** Unleſs the chemical reader makes this liſt very familiar to his mind, he muſt remain incapable of reading with intelligence the writings of all the chemiſts of Europe previous to the publication of Mr. Lavoiſier's ſyſtem. Yet thoſe writings contain a vaſt body of ſolid chemical knowledge, all of which has a chance of being forgotten, if it have not an immediate and palpable dependence on the new doctrines. He will remain equally ignorant of the writings of the moſt eminent natural hiſtorians and mineralogiſts, who employed no other language in all their diſcuſſions. EDITOR.

NOTES AND OBSERVATIONS

BY THE EDITOR.

[*Note* A. *p.* 59. *bottom.*]

The reader will ſurely be ſenſible that the ſources of error in the experiments for aſcertaining this fundamental point are numerous, and are with difficulty avoided. We are not entitled to make a deciſive inference from the experiments, unleſs they be numerous, and their unavoidable irregularities be as frequently on one ſide as on the other of the general inference. It was a natural expectation that equal increments of bulk ſhould accompany equal increments of heat; yet we cannot ſay that this has a good foundation. Ignorant of the intimate nature of heat, we are ignorant of the manner and the force with which it is accumulated round the particles of a body. But, on the other hand, the obſervation of the variations of bulk afford us the beſt means of diſcovering thoſe hidden circumſtances. Philoſophers acquieſced too readily in the opinion that the increments of bulk were proportional to thoſe of heat. The experiments of Dr. Brook Taylor, made with great judgment, ſeemed to confirm this opinion. Thoſe of Rhenaldini did not contradict it. Sir Iſaac Newton's experiments, proceeding on this ſuppoſition, combined with a moſt ingenious ſuppoſition concerning the progreſs of heating and cooling, exhibited a happy coincidence of facts, depending on principles totally different, which ſeemed to put this opinion beyond all doubt. Dr. Black's experiments in 1760 perfectly coincided with it.

But Mr. De Luc, one of the moſt aſſiduous, and perhaps moſt ſcrupulous philoſopher of Europe, had more than doubts about the certainty of this correſpondence. He had noticed the obſervation of Beaumé, that water not only increaſed greatly its bulk in freezing, but that its contraction by cold ceaſed *before* its converſion into ice, and that it even expanded a little. He verified this, and aſcertained its quantity by careful experiments, of a very ſimple and unexceptionable kind, namely, by comparing the contractions of a quickſilver and a water thermometer. This being the caſe, there would be great uncertainty in the indications of temperature by a water thermometer, or one containing any water in its compoſition, ſeeing that there muſt be a condition of greateſt denſity; and therefore, on each ſide of the correſponding temperature, the water will have the

ſame bulk in very different temperatures. The extent of this uncertainty is unknown The curious experiments of Captain Williams at Quebec, in which he froze water in bomb-ſhells, ſhew that *before* it congeals, it expands prodigiouſly and *gradually*. This muſt be inferred from the expulſion of the iron plug, and protruſion of a long cylinder of ice, in caſes when the ſhell did not burſt. It were worth examination what would be the progreſs of its expanſion, while cooling undiſturbed, below the ordinary freezing temperature. But are we certain that during this expanſion of water in temperatures below 38°, the water is giving out heat in the ſame manner as during its contraction, in cooling from a ſtate of higher temperature? Mr. De Luc now ſaw the neceſſity of following out this compariſon through the whole range of the thermometer. For, ſince there is a temperature a little above that of melting ice, in which a change of heat makes no change in the bulk of water, it neceſſarily follows that equal variations of temperature on both ſides of this degree of heat, will be accompanied by variations of bulk, which continually increaſe. Accordingly, he found that while the mercurial thermometer increaſed its bulk equably, the increments of the bulk of the water continually increaſed. But, farther, when he endeavoured to determine the proportion of this augmentation of the ſucceſſive increments of the water, correſponding to equal increments of the mercury, he found that when the temperature approached to that of boiling water, theſe augmentations of bulk increaſed with increaſing rapidity, and were remarkably irregular. He found the ſame thing in ſpirit of wine and other fluids. Some fluids, however, ſuch as mercury, olive oil, lintſeed oil, oil of camomile, and others, contract in freezing, and are not ſubject to all this irregularity. In ſuch fluids, the acceleration of their expanſion by heat begins at the very departure from the ſtate of ſolidity. Hence Mr. De Luc concludes that their expanſion is more correſponding to the changes of heat, in proportion as they are farther from either the freezing or the boiling temperatures.

Mr. De Luc's reaſoning on this ſubject is in ſome degree hypothetical, depending on his notions of the relation of bodies to heat. But he has eſtabliſhed beyond diſpute the fact, that the ſucceſſive augmentation of bulk by equal increments of heat are continually increaſing. Mr. De Luc makes one inference which cannot be controverted, and is of importance to the philoſopher, namely, that is the beſt fluid for a thermometer, which has the greateſt interval between its freezing and boiling temperatures; becauſe the ſucceſſive augmentation of bulk towards the middle of its range will the moſt nearly correſpond with the augmentations of heat. Were it not that there is now no difference of opinion, this circumſtance alone would juſtify the preference which De Luc gives to mercury above all other fluids. Its deviations from equable expanſion by equal changes of temperature, in the caſes which moſt frequently occur in our experiments, are ſo trifling, and indeed ſo uncertain, that they need ſcarcely be attended to. They are called uncertain, becauſe it is plain that the nature of the experiments for aſcertaining the real medium temperature, ariſing from the mixture of a hotter and a colder fluid, ſeems unſuſceptible of the accuracy neceſſary for determining ſuch ſmall quantities with preciſion.

The annexed table of Mr. De Luc's compariſon of thermometers filled with different fluids cannot but be acceptable.

He had determined the expanſion of mercury by very careful experiments, and found that in the middle temperature between thoſe of freezing and boiling water, that is, at 40° of Reaumur's thermometer, the mercurial thermometer ſtood at 38,6; and the others, whoſe ſcales were all of equal parts, containing 80 between theſe two temperatures, ſtood as follows:

Mercury,	38,6	15 : 14
Olive and lintſeed oil,	37,8	15 : 13,4
Oil of camomile,	37,2	15 : 13
Water ſaturated with ſalt,	34,9	15 : 11,6
Rectified ſpirits of wine,	33,7	15 : 10,9
Water,	19,2	15 : 4,7

The ſecond column expreſſes the proportion of the contraction from the temperature of boiling water, to the middle temperature 40°, to the contraction from 40° to 00°.

[*Note* B. *p.* 63. *line* 5.]

This method of meaſuring intenſe heats is extremely refined, and could occur only to ſuch a mind as Newton's. It is, however, in ſome reſpects hypothetical and gratuitous, becauſe it proceeds on a ſuppoſition which I apprehend cannot be demonſtrated, namely, that the momentary emanations of heat, whether in the way of communication by contact, or by ſimple emiſſion, or in both ways, are proportional to the exceſs of the temperature of the cooling body over that of the ſurrounding or receiving bodies. We know nothing of the connection of bodies with heat, from which this law of variation can be properly inferred. But, on the other hand, when we obſerve the progreſs of heating and cooling, in caſes where we have other evidences of the quantities gained or loſt, we find it wonderfully correſpondent to this law, ſo much ſo indeed, that we cannot perceive any deviation from it which may not be accounted for by the circumſtances of the caſe. This method, therefore, is a very valuable acquiſition to the philoſopher.

[*Note* 1. *p.* 84.]

It is ſomewhat ſtrange, that neither Boerhaave, who expreſſes great curioſity about all the relations of bodies to heat, nor the great number of ingenious men who were much occupied with the ſtudies connected with thoſe relations, were induced, by the original experiment of Fahrenheit on a mixture of water and mercury, to examine ſo important a queſtion as that concerning the proportion of heat in different ſubſtances having the ſame temperature. The experiment lay unnoticed, or was copied out by many com-

pilers, without taking the trouble of thinking about it. Dr. Martin, alſo, though engaged in experiments which were very cloſely allied to this queſtion, did not attend to it. The philoſophers on the Continent ſeem agreed in giving the honours of the diſcovery of the unequal diſtribution of heat among bodies of different kinds, to Profeſſor Wilcke of Stockholm, who read a diſſertation on it in the Royal Academy in 1771. (*Swed. Tranſ. vol.* 35. *p.* 93. *New Swed. Tranſ.* 1782, *Part Second.*)

But Dr. Black had ſeen its importance long before, and had diſcovered many caſes of very unequal diſtribution. In ſome ſmall note books, written while a ſtudent of medicine in Edinburgh, he ſets down queries, founded on Fahrenheit's experiment, and projects of experiments, on a variety of ſubſtances which had exhibited puzzling appearances in this reſpect. He had made ſeveral of theſe before 1757, and, among theſe, ſome which muſt have given riſe, in his thoughts, to his celebrated theories of fluidity and vapour.

Mr. Watt of Birmingham was, as far as I can learn, the firſt who conſidered this ſubject ſteadily, and in a ſyſtem. He was ſtrongly incited to this examination by the great object which then occupied his attention,—the improvement of the ſteam engine. From Dr. Black's lectures he had learned the doctrine of latent heat, and ſaw that a great obſtacle to the performance of that noble engine was the waſte of ſteam by ineffective condenſation. It was, therefore, an intereſting thing to him, during his firſt ſpeculations, to learn the quantity thus waſted by warming the cylinder and other parts of the apparatus; and ſome of his earlieſt attempts to improve the engine conſiſted in employing cylinders made of ſubſtances which required little heat for warming them. He made many experiments on various ſubſtances, in order to learn their properties in this reſpect. They were but rude, but ſufficient for his purpoſe; and the examination would have been carried much farther, had he not contrived the ſeparate condenſer, which made this property of bodies of little moment to him.

Mr. Watt's experiments were all communicated to his preceptor, who took the warmeſt intereſt in them; and this correſpondence gave him the opportunity of knowing the uncommon genius and the great worth of his pupil, and was the beginning of a friendſhip which laſted through life, and which, for the merit of the parties, and for mutual confidence, reſpect, and attachment, has had few equals.

Before the year 1765, Dr. Black had made many experiments on the heats communicated to water by different ſolid bodies, and had completely eſtabliſhed their regular and ſteady differences in this reſpect. He mentioned theſe in his lectures in the Univerſity of Glaſgow, and was occupied with the experiments for the ſame purpoſes when he was called to Edinburgh. He was much aſſiſted in the experiments by Mr. Irvine, who afterwards lectured from the ſame chair. The regiſters of theſe experiments remain among Dr. Black's papers, moſt of them dated, and partly written by himſelf, and partly by Mr. Irvine. They are all previous to 1770. Mr. Irvine made many experiments on the ſame ſubject, beſides thoſe in which he aſſiſted Dr. Black; and particularly on thoſe

fluids which, without any remarkable chemical action, produce heat by mixture; ſuch as water and vitriolic acid, water and ſpirits of wine, &c. And it was this train of experiments which led Dr. Irvine to that ingenious and plauſible method of determining the point of abſolute privation of heat, which he publicly taught in his lectures, and communicated to his philoſophical friends. His fundamental propoſition was, that "the "heat which appeared in mixing vitriolic acid and water is the difference between the "ſum of the abſolute heats of the two ingredients, and the abſolute heat of the mixture, "while the heats, which each of them ſeparately required for an equal variation of tem-"perature, had the proportion of their reſpective abſolute heats." Therefore, having diſcovered, by ſuch experiments, the difference, and the ratio of the abſolute heats of the ingredients, we can find thoſe abſolute heats, and the temperature at which thoſe heats commence, or in which the ingredients contain no heat at all.

As Dr. Black and Dr. Irvine now lived at a diſtance from each other, and each was aſſiduouſly occupied with his official duties, and intereſted in acquiring a deſerved reputation, their mutual communications were leſs frequent than formerly, but they were by no means given over. Dr. Black's ſpeculations were anxiouſly directed to one favourite object,—the theory of fluidity and vapour; and Dr. Irvine's to the diſcovery of the abſolute heats of bodies. It was in the proſecution of this inveſtigation that Dr. Irvine found himſelf obliged to uſe ſome appropriate terms for expreſſing his peculiar views of the ſubject. That quality by which ſome ſubſtances required more heat than others to produce the ſame elevation of temperature, had different names at different ſeaſons during his progreſs. *Affinity* pleaſed him a long while, and was thus employed both by Dr. Black and Dr. Irvine. Both alſo uſed the terms *capacity*, *faculty for receiving heat*; *appetite for heat*, &c. &c. *Capaeity* was at laſt adopted, as the beſt ſuited to all the occaſional modifications of this property which appeared in the experiments. It was the familiar language among the ſtudents of chemiſtry, both in Glaſgow and Edinburgh; and I have not the leaſt doubt but that the Swediſh gentlemen, of whom there were ſeveral at Glaſgow and Edinburgh between 1763 and 1770, carried home with them both the doctrines and language which were familiar to them. Theſe facts, which are all conſiſtent with my perſonal knowledge, being myſelf on the ſpot, are of ſome uſe for aſcertaining the value of many claims of originality and priority which have been too warmly diſputed. The great merits of a Wilcke, a Bergmann, a Lambert, are not diminiſhed, although we learn that Dr. Black and Dr. Irvine had preceded them in ſome of their ingenious reſearches.

Before concluding this note, it will not be unacceptable if the general view of this doctrine of the *ſpecific capacities* of bodies for heat be communicated in a compendious manner, and the method of applying the principle to the chief queſtions which may be propoſed, be briefly deſcribed.

It is aſſumed, in the firſt place, that the proportion of the quantities of heat required

for changing the temperature of the two bodies equally, is conſtant. This is not unlikely; but it is by no means demonſtrated: Nay, the experiments of Dr. Black and Dr. Irvine on bees wax, ſpermaceti, &c. directly prove the contrary; in all caſes at leaſt where the body becomes gradually ſofter and ſofter before melting. We are not certain but that ſomething of this may obtain in moſt ſubſtances. Lichtenberg thinks that water itſelf becomes more perfectly fluid by increaſing its heat. But the change obſerved by him was ſo trifling as not to cauſe any ſenſible variation in its comparative capacity, in all the temperatures between freezing and boiling.

Dr. Black eſtimated the capacities, by mixing the two bodies in equal maſſes, but of different temperatures; and then ſtated *their capacities as inverſely proportional to the changes of temperature of each by the mixture.* Thus, a pound of gold, of the temperature 150°, being ſuddenly mixed with a pound of water, of the temperature 50°, raiſes it to 55° nearly: Therefore the capacity of gold is to that of an equal weight of water as 5 to 95, or as 1 to 19; for the gold loſes 95°, and the water gains 5°.

It will be moſt convenient to compare all bodies with water, and to expreſs the capacity of water by unity, or to call it 1. Let the quantity of the water be W, and its temperature w. Let the quantity of the other body be B, and its temperature b. Let the temperature of the mixture be m. The capacity of B is $\frac{W \times \overline{m-w}}{B \times \overline{b-m}}$ Or, when the water has been the hotter of the two, the capacity of B is $\frac{W \times \overline{w-m}}{B \times \overline{m-b.}}$ In other words, *multiply the weight of the water by its change of temperature. Do the ſame for the other ſubſtance. Divide the firſt product by the ſecond. The quotient is the capacity of the other ſubſtance, that of water being accounted* 1.

Theſe experiments require the moſt ſcrupulous attention to many circumſtances which may affect the reſult. 1. The mixture muſt be made in a very extended ſurface, that it may quickly attain the medium temperature. 2. The ſtuff which is poured into the other ſhould have the temperature of the room, that no change may happen in the pouring it out of its containing veſſel. 3. The effect of the veſſel in which the mixture is made muſt be conſidered. 4. Leſs chance of error will be incurred when the ſubſtances are of equal bulk. 5. The change of temperature of the mixture, during a few ſucceſſive moments, muſt be obſerved, in order to obtain the real temperature at the beginning. 6. No ſubſtances ſhould be mixed which produce any change of temperature by their chemical action, or which change their temperature, if mixed when of the ſame temperature. 7. Each ſubſtance muſt be compared in a variety of temperatures, leſt the ratio of the capacities ſhould be different in different temperatures.

When all theſe circumſtances have been duly attended to, we obtain the meaſure of the *capacities* of different ſubſtances for heat.

Philoſophers do not ſeem agreed as to the meaning affixed to this term. It is frequently uſed in the ſenſe of mere *capaciouſneſs*, or *room* for the admiſſion of heat. But this is a very needleſs conſideration, and unconnected with all chemical ſcience. Heat is

not merely *admitted* into bodies, but *abſorbed* or *drawn* into them, in conſequence of ſome tendency to combination, whatever the nature of this union may be; whether mere adheſion, like capillary attraction, or the mixture of metals or of watery fluids; or like the ſolution of ſaline cryſtals in water, and the mixture of brines with more water; or like the union of ſalt and water in cryſtallization; or like the union of acids and alkalis. In all theſe examples, there is ſome phyſical action, and this is continued till an equilibrium of the acting forces is effected. This is very different from mere capaciouſneſs, and it even receives no explanation by the help of capaciouſneſs.

The only thing expreſſed by the numbers above mentioned is the *variability of temperature by heat*.

Dr. Crawford, in his eſſay on animal heat, has given tables of the capacities of a great number of ſubſtances. But a want of attention to ſome of the above mentioned cautions, particularly the ſixth *, has led him into many miſtakes; and therefore, in a ſecond edition he has made very great changes in his meaſures. But even his ſecond meaſures ſeem exceedingly gratuitous and uncertain. Mr. Kirwan has alſo aſcertained the capacities of a conſiderable number of bodies. His table is publiſhed in *Magellan's diſſertations*. Bergmann has given another table in his *eſſays on elective attraction*. Baader has given another in his *Diſſertation on Heat*; and, laſtly, Gadolin has given a large liſt, in his *Theory of the Specific Heat of Bodies*, *Abo*. 1784.

Wilcke calls the number ariſing from *our* formula, the meaſure of the *ſpecific heats*. But if the quantities of the ſubſtances are reckoned by their bulk, then the reſulting numbers are the meaſures of what he calls the *relative heats*. The relative heats may be obtained by multiplying the ſpecific heats by the ſpecific gravity of the ſubſtance compared with water in the experiment. Thus, the relative heat of gold is 1, or equal to that of water; while its ſpecific is only 0,0526, or $\frac{1}{19}$. The compariſon is moſt frequently made by meaſures in bulk, in which caſe the formula gives the relative heats. But Wilcke thinks the meaſures by weight more proper, becauſe the queſtion ſeems to relate chiefly to the peculiar nature of the matter itſelf, independent of all conſideration of ſpace.

Dr. Irvine, Dr. Crawford, and moſt of the other writers on this ſubject, have conſidered thoſe numbers as expreſſive of the proportions of the abſolute quantities of heat, or cauſe of heat, contained in bodies, when they are all of one temperature. Indeed, it is this opinion which gives the chief importance to the whole doctrine of ſpecific heats. But this opinion is juſt, only on the ſuppoſition that the meaſures, obtained by the above deſcribed experiments and calculation, are conſtantly the ſame, whatever the temperatures may be in which the experiments are made. Dr. Irvine's ingenious method of diſcovering the temperature of abſolute privation, evidently preſuppoſes this conſtancy of ſpecific heat;

* Dr. Crawford was particularly intereſted in the ſpecific heats of the ſubſtances uſed by animals for food. Moſt of theſe generate heat or cold, when mixed with water. Flour of all kinds does this in a remarkable degree. Even different kinds of flour generate heat, by mixing them ſuddenly together in great quantities.

or, if they are not conſtant, it ſuppoſes that we know the whole law of variation. Now both of theſe aſſumptions are highly improbable. In none of the progreſſions of natural operations that we are acquainted with do we find this conſtancy. It is much more analogous to other phenomena, to ſuppoſe that, in the temperatures near to that of abſolute privation, the quantities of heat neceſſary for producing equal elevation gradually diminiſh, and this, perhaps, without end, like the diſtance of the hyperbola from its aſſymptote. It is equally probable that the law of diminution may be different in different ſubſtances. This will cauſe the meaſures of ſpecific heats to change their proportions continually; and therefore the ſpecific capacities obſerved in temperatures, all of which are far removed from that of the entire abſence of heat, give us no means of obtaining the proportions of the accumulated ſum of all the heats which has been received into the ſubſtances. It follows from this, that even although it ſhould be granted to Dr. Irvine, that the heat which emerges, in mixing vitriolic acid and water, or in the freezing of water, is the difference between the abſolute heat of the mixture, or the ice, and the abſolute heats of the ſubſtances before mixture, or of the water before freezing, ſtill we cannot aſcertain thoſe abſolute heats, or the temperature of no heat.

Accordingly, it appears that it has been only in a very few caſes that Dr. Irvine found a tolerable coincidence of his determination of this extreme cold; and the determination by means of mixtures, differed enormouſly from thoſe obtained by means of congelation; and ſtill more from thoſe obtained by means of the condenſation of vapour. The obſervations of Gadolin on theſe experiments are highly inſtructive and judicious.

Upon the whole, it appears that, although the informations which we have acquired by thoſe experiments muſt be, on many occaſions, very uſeful, yet they give us little aſſiſtance for determining either the proportions, or the real quantities of the matter or cauſe of heat contained in bodies, or the temperatures in which any body contains no heat at all. It is not even certain that adjoining bodies will all acquire one temperature. In ſhort, we are as yet too little acquainted with the nature of heat, and the laws of its connection with other ſubſtances, to pronounce with confidence on any of theſe queſtions. We muſt be better informed concerning the mutual relation of the particles of this ſuppoſed ſubſtance, and concerning the powers or forces which combine it with other matter. There muſt, in every quieſcent or permanent condition of a body with reſpect to heat, be an equilibrium between all theſe forces and the forces which connect the particles of other matter. This is a ſubject more of a mechanical than chemical nature. The chemiſt will not perhaps derive much advantage from a perfect knowledge of the ſubject; and he will contribute more to the advancement of his own ſcience by overlooking this reſearch, and attaching himſelf to the general though complex facts that occur in his experiments. Eſtabliſhing a general fact in chemiſtry is obtaining a fixed point, from which we can ſet out on any farther ſearch. In this reſpect, the tables of ſpecific or relative heats, or of capacities, are real additions to chemical ſcience, and will often aſſiſt us in the explanation of phenomena ſtill more complicated, although we ſtill remain ig-

norant of the principle that unites them all. Newton eſtabliſhed a noble body of aſtronomical ſcience, although he profeſſed total ignorance of the nature of gravity. Dr. Black, ever more deſirous of communicating truly chemical knowledge than refined philoſophical doctrines, has generally avoided all ſuch diſquiſitions in his lectures. Theſe were intended for the information of even ſuch as had no previous ſcientific education; and it was always with ſome reluctance that he engaged in abſtruſe ſpeculations.

[*Note* 2. *p.* 86.]

It muſt be acknowledged, however, that this proportion is not ſupported by any very preciſe experiments, although ſuch are neither abſtruſe nor difficult. There are even circumſtances well known to us, which cannot but have a notable influence on the heating and cooling of bodies. We know that heat is much more ſpeedily communicated to the remoter parts in ſome bodies than in others, when the heating ſubſtance is applied to one extremity of the body. Dr. Black takes particular notice of this diſtinction in a ſubſequent part of his lectures. This being the caſe, it muſt follow that bodies which conduct heat ſlowly along their own ſubſtance, muſt alſo imbibe and emit it ſlowly, becauſe their ſuperficial parts tranſmit ſlowly what they have received from the heating cauſe to the interior parts. The imbibition and emiſſion of heat will alſo depend, for the ſame reaſon, on the conducting power of the heating or cooling medium.

But, beſides this obvious cauſe of difference, there are circumſtances of a more abſtruſe nature, which render the truth of this propoſition very doubtful, and make it neceſſary to have it evinced by very unqueſtionable experiments. There are two ways in which heat is received or emitted by a body. Firſt, by communication in contact with the body from which it comes, or into which it goes. The tranſmiſſion ariſes from the want of that equilibrium of connecting forces, which produces a permanent condition of things. Now, ſince we find that different bodies contain different proportions of this moveable heat, it is plain that the equilibrium now mentioned muſt be broken in different degrees by the addition or abſtraction of the ſame abſolute quantity of heat; and therefore the quantity which will eſcape or come in, in equal times, ſo as to produce a new equilibrium, will be different,—that is, that heat will not be abſorbed or emitted with equal celerity by all bodies.

In the next place, heat is unqueſtionably emitted from bodies alſo in the manner of light, or by what may be called *radiation*. This is performed with immenſe velocity, ſo as to ſeem inſtantaneouſly darted to any diſtance. A late writer indeed has aſſerted, in *Nicholſon's Philoſophical Journal for November* 1800, that heat is propagated only by communication through the air, and therefore ſlowly. But this is contrary to moſt diſtinct and varied experiment. The very curious obſervations of Dr. Herſchell ſhew that heat is radiated in the ſame manner as light, (*Phil. Tranſ.* 1800.) and is refracted and re-

flected alſo in a ſimilar manner. It may here be obſerved, by the way, that this obſervation of the conſtant ratio of the ſines of incidence and refraction, is the ſtrongeſt argument that has yet been obtained for the materiality of heat. For this law of motion is competent only to a material particle, having mobility and inertia, and acted on by tranſverſe accelerating forces, in the ſame manner that a cannon ball is acted on by gravity, and by the force of projection. Dr. Scheele is conſidered as the firſt perſon who obſerved and eſtabliſhed this radiation of heat, and its ſeparability from light. But it was obſerved much more than a century ago by Dr. Robert Hooke, and exhibited by him to the Royal Society at the meeting of May 15, 1682. (*Birche's Hiſtory*, IV. 137.) Mr. Pictet of Geneva alſo has made ſome remarkable obſervations and experiments, which ſhew that heat is continually emitted from all bodies warmer than the medium, although by very ſmall degrees. This is confirmed by the direct obſervations of Dr. Herſchell.

Now in this caſe, as we are entirely ignorant of the forces which expel this heat from the body, and of their laws of action, and of the proportion of the heat radiated to that carried off by contact, and whether this proportion be conſtant or variable, it ſeems unwarranted to aſſume this propoſition as true, without any direct proof from obſervation. We have no ſuch proof, nor indeed any obſervation of the comparative celerity from different bodies, ſufficiently preciſe to give us diſtinct knowledge either of its truth or falſehood.

[*Note* 3. *p.* 100.]

We muſt not omit taking notice of a very ſingular diſtinction, which Count Rumford has endeavoured to eſtabiſh between the conducting power of ſolid and fluid bodies, namely, that while heat is propagated along ſolid bodies, from particle to particle, according to certain regular laws, fluid bodies are heated in their remote parts only by the actual transference of the particle which received heat from the hot body when in contact; and that, without this motion of the heated particles, occaſioned by their expanſion, and conſequent levity, the remote parts of a fluid would never be warmed. The ingenious and worthy author has ſupported this opinion by a ſeries of curious and well contrived experiments, the reſults of which are moſt unexpected and ſurpriſing. They are to be ſeen in the 7th of his eſſays.

Dr. Black was much ſtruck with the novelty and importance of this opinion, and began to examine it with that cautious exactneſs which he had always practiſed; but the great decline of his health and ſtrength ſoon checked his progreſs, and the few notes on the ſubject which are to be found among his papers, are inſufficient for giving any notion of his ſentiments.

It is clearly eſtabliſhed by the experiments of Count Rumford, that when the internal motion, producible by the inequality of ſpecific gravity, occaſioned by heating the lower parts of a fluid, is prevented, the progreſs of heating is almoſt inſenſible, and is no more

than what we may expect by means of the ſolid matter of the veſſel, and by radiation. It is alſo certain, that our knowledge of the relation or connection of a particle of matter to heat, gives us no information of the manner how heat attached to one particle is ſhared with or communicated to another. It is the perfect equality of action in all directions, which fits a particle of matter for being the particle of a fluid; while a certain polarity, ariſing from an inequality of action in different directions, would fit it for being the particle of a ſolid. It is not at all unlikely that this difference may ſo change this attraction (or whatever name is given to the relation) of a particle for heat, that a ſuperfluity of heat may have a tendency to quit the *pole* of one particle for the *pole* of another, in a ſolid body, while there is no ſuch tendency with reſpect to a particle of a fluid, having no polarity. Heat may continue to accumulate ſpherically round the latter, expanding the molecula conſiſting of ſuch particles, without leaving it for another. We ſee a difference of relation not leſs diſtinct and remarkable, in the caſe of liquids and gaſes. Liquids, like ſolids, expand in arithmetical progreſſion, acquiring equal additions of bulk by equal additions of heat; whereas gaſes, ſuch as common air, carbonic acid, &c. expand in geometrical progreſſion by equal elevation of temperature. An increaſe of 30° nearly doubles the bulk of ordinary ſteam; another 30° doubles this bulk, or 60° quadruples the original bulk nearly. It may be added, that the phenomena of galvaniſm ſhew us ſtill another modification of the chemical relations of atoms, by which a quality or power of producing effects is propagated to any diſtance, without our being able to conceive that any material ſubſtance is tranſported along this line of viſible activity.

There is nothing, therefore, in the opinion offered with great caution by Count Rumford, that is incompatible or inconſiſtent with our moſt diſtinct notions on theſe ſubjects. It is a fair field of inquiry, moſt ingeniouſly laid open to philoſophers. It even promiſes to aſſiſt us in acquiring juſter notions of chemical action, than thoſe which at preſent ſatisfy chemiſts, and they would do well to ſtudy it mathematically, as P. Boſcovich has done. This would ſoon make them exclude ſome modes of action which are in faſhion, and by thus limiting the attention, would ſooner bring us into the right path.

[*Note* 4. *p.* 144.]

The celebrated Bergmann having learned and adopted Dr. Black's theory of fluidity, imagined that this abſorption of heat in a latent ſtate would give an excellent method for examining the queſtion ſo much debated by philoſophers, "Whether heat be ponderous "or not?" The experiments which had been made for this purpoſe have been very anomalous, by reaſon of many unavoidable ſources of inaccuracy, in all caſes where great quantities of ſenſible heat are accumulated in bodies, in order to make its weight perceptible. Currents of air are produced, which affect the ſcales of the balances employed. The great heats expand the arms of the balance, which deſtroys the equilibrium, and

affects the meaſurement. The very expanſion of the body affects it, by changing its ſpecific gravity.

Bergmann propoſed to weigh, not the ſenſible heat, but the latent heat, which is accumulated in conſiderable quantities, in ſuch a manner as to be free from errors. He propoſed to weigh a maſs of water, of the temperature 32° or 33°, in a room having this temperature, and then to freeze it completely; and, having again brought the ice to the temperature 32°, to weigh it again in a room having that heat. Thus all the cauſes of error ſeem to be avoided.

This judicious experiment has been made with the moſt ſcrupulous care by Dr. Fordyce, (*See Phil. Tranſ.* 1785.) with about three and a half ounces of water, contained in a thin glaſs globe hermetically ſealed. Its weight was examined by a balance which was ſenſible to the $\frac{1}{1000}$th part of a grain, and it was *repeatedly* found $\frac{1}{10}$th of a grain, nearly $\frac{1}{35000}$th of the whole lighter when in the fluid ſtate. Similar experiments have been made, having the ſame reſult, by Sir Benjamin Thomſon, (now Count Rumford) by Mr. Gouvenain at Paris, and by others. Dr. Fordyce alſo found that the ice became $\frac{1}{10}$th of a grain heavier, when cooled down to 22°. This might be expected from the contraction of bulk, and from the downward current of air which ſo cold a maſs would occaſion.

Dr. Black ſeems diſpoſed to aſcribe this unexpected effect to a repulſion for the heat in the earth. This, however, conſidered as the weak exertion of the repellency which he ſuppoſes to be a property of the atoms of heat, is not conſiſtent with ſuch laws of relation, whether attraction or repulſion, as are neceſſary for the corpuſcular combinations of matter. Theſe muſt decreaſe at leaſt in the inverſe triplicate ratio of the diſtances, and be altogether inſenſible at any meaſurable diſtance. But it is much more reaſonable to aſcribe the ſmall diminution obſerved, the 16th part of the 17000th part of the whole, to errors unavoidable in ſuch experiments. Accordingly, Count Rumford, ſome years after, repeating the experiments on much larger quantities of materials, could obſerve no difference whatever. Nor ſhould we expect to find heat ponderous. When we recollect that it comes with the ſame velocity as light from the ſun, and yet exerts no ſenſible impulſive force, we have no reaſon to ſuppoſe the quantity of matter to have a ſenſible weight.

[*Note* 5. *p.* 197.]

The worthy author of theſe lectures was always more anxious to communicate what may be called a clear and confident knowledge of the doctrines of pure chemiſtry, than to lead his pupils into abſtruſe or refined ſpeculations on the unſeen and unknown immediate cauſes of chemical combination. He conſidered every queſtion of this kind as rather out of the pale of chemical ſcience; and ſo it certainly is. Whenever we ſpeculate about the attractions and repulſions of particles, as the immediate agents in effecting

the chemical changes, we are no longer chemiſts, but mechanicians. We are conſidering queſtions about local motion, and the mathematical determinations of the effects of moving forces. Not only is the occupation not chemical, but the queſtions themſelves give little addition of chemical knowledge. This, at leaſt in the preſent ſtate of the ſcience, is promoted chiefly, if not ſolely, by a more ſucceſsful generalization of chemical phenomena. This generalization is, in fact, the eſtabliſhing of general laws, and marking with greater accuracy their diſtinctions and limitations. But the chemical phenomena which are to be claſſed are all of a very complicated kind; and it is of infinitely more conſequence to know the full extent, and all the modifications of the law, than to know the hidden proceſs of nature in any one of the phenomena; juſt as the noble ſyſtem of phyſical aſtronomy, which Newton was able to claſs under one general fact (univerſal gravitation in the inverſe duplicate ratio of the diſtance) was of more importance than the explanation how this fact was effected.

In all caſes, therefore, Dr. Black contented himſelf with preſenting his ſubject in the ſimpleſt form, ſuggeſting the ſimpleſt notion of it. Nothing could be more ſimple than his doctrine of latent heat. The experience of more than a century had made us conſider the thermometer as a ſure and an accurate indicator of heat, and of all its variations. We had learned to diſtruſt all others. Yet, in the liquefaction and vaporization of bodies, we had proofs uncontrovertible of the entrance of heat into the bodies. And we could, by ſuitable proceſſes, get it out of them again. Dr. Black ſaid that it was concealed in them,—*latet*,—it was as much concealed as carbonic acid is in marble, or water in zeolite,—it was concealed till Dr. Black detected it. He called it LATENT HEAT. He did not mean by this term that it was a different kind of heat from the heat which expanded bodies, but merely that it was concealed from our ſenſe of heat, and from the thermometer.

It has always ſurpriſed me that a doctrine ſo plain, ſimple, and unaſſuming, ſhould be miſunderſtood. For it muſt be miſunderſtood to be contradicted in any ſhape. Dr. Black would not have thought any more about a ſubject already ſo plain, nor would he have paid any attention to the condition or ſtate in which this heat exiſts in the body, had he not been called upon by thoſe who ſaid that he had introduced an unneceſſary principle into the doctrine of heat, and had he not heard it aſſerted that the heat was no more latent than while it expands a body. He knew this, but aſſerted that the indication of its preſence, though undiſputable, was not heating, or expanding, but melting a body, or converting it into vapour. It was only in order to anſwer this challenge that he was led to conſider its peculiar condition, by which its mode of action was rendered ſo different from what it was before. His firſt obſervation was, that it was no longer ſo free and moveable as before, and that it was only the *additions* made to the heat neceſſary for mere fluidity which were now moveable, and affected the thermometer. He now ſaw good chemical reaſons for conſidering this concealed heat as *united* with the ſubſtance of the body, in a way very much reſembling many chemical combinations. A warm

body carried its warmth about with it. It ſhared its warmth with other bodies, juſt as a brine ſhares its ſalt with more water. In liquefaction and vaporization it attached to itſelf a determined quantity, and no more than the proper quantity, juſt as water does of ſalt, or as ſalt does of water in forming a cryſtal. The appearance, and many properties and modes of activity, are changed by this union, juſt as in other chemical combinations. He was therefore led to conſider heat as chemically combined, in all its modes of exiſtence, whether in expanding, in melting, or in boiling off a fluid. He conſidered heat in a certain proportion as an *ingredient* of all liquids, and, in another proportion, as an ingredient in all vapours.

But all this was refuſed, and the combination of heat was denied, merely becauſe the touch of a cold body could take out this heat,—becauſe the combination is not permanent, like the generality of chemical combinations; and it was aſſerted that this great quantity of heat was merely *received* into the body, and lodged there, without affecting the thermometer, merely becauſe the body, by its change of form, had more room for it.

Dr. Black now ſaw ſtrong reaſons for adhering ſtill more to the conception he had now formed of its mode of exiſtence in the fluid and vapour. He now conſidered heat as the *active cauſe* both of fluidity and of vapour, producing thoſe new modes of aggregation by a true chemical combination with the particles of the body. To the reaſons adduced by Dr. Black for this opinion I would add one or two more, which I think are incompatible with the notion of mere capacity.

When a piece of dry ſponge lies in the exhauſted receiver of an air-pump, and we admit the air into the receiver, it ſoon fills all the pores of the ſponge. This has capacity for its reception, in the pureſt meaning of the term. The ſponge exhibits no appearance of change upon this reception of air into its pores. But, let us pour water into the receiver, the ſponge has the ſame capacity or room for the water. But how different the appearance! The ſponge ſwells to twice its former bulk. And, although we underſtand very little of the corpuſcular actions of bodies, we comprehend a good deal of this proceſs. We know that the ſponge does not merely *admit* the water into its pores; it *attracts* it, and will even raiſe the water to a conſiderable height, if one end only of the ſponge be dipped into the water. We even comprehend a little of the manner in which this attraction produces the expanſion of the ſponge. There is a force, which is mutual between the ſponge and the water, acting at the mouth of the pore, urging the water into it, preſſing it on the water already in, in the ſame manner as if a little piſton were preſſing on the water in the mouth of the pore, and therefore diſtending the pore. We can even calculate the limit to the effect of this force, and what rigidity of the pore will put a ſtop to the accumulation. We know that this attraction will keep the bodies united. And we ſay, with the utmoſt propriety, that they are united—combined—and we call this particular mode of union *adheſion*. And, laſtly, we ſay that the water is the cauſe of this diſtenſion of the ſponge. It would be very aukward to ſay that the capacity of the ſponge

is the cauſe of this diſtenſion. It is very true that the cauſe of it is not the water, but the force which unites the water and the ſponge. But the water is the occaſion or cauſe, in the ſame manner that fixed alkali is the cauſe why lime is ſeparated from its ſolution in an acid. If the experiment had been made with a piece of dry wood in the place of ſponge, we know that it would have ſwelled with a force ſufficient for ſplitting the firmeſt rocks. It would ſurely be ridiculous to aſcribe this to the capacity of the wood. If any perſon, unaccuſtomed to diſcuſſions of this ſort, finds a difficulty in conceiving how attraction, which rather ſeems fitted for contracting the ſponge, produces its expanſion, he may ſee this effect of undoubted attraction exerted between the particles of mercury. Lay two or three ſmall globules of mercury on a ſmooth plate of glaſs: There can be no doubt of the mutual attraction of the atoms being the cauſe of the round form of the drops. Lay another light plate of glaſs on theſe drops of mercury: Its weight will flatten them a little. Lay on ſome ſmall weights: This will flatten them ſtill more. The addition of more weight will increaſe this effect. Now lift off the weights one after another. The drops will gradually contract their breadth again, *by raiſing the upper plate farther from the under one.* Here it is plain that the mutual attraction of the particles forces the plates aſunder.

This reaſoning is applicable, in the ſtricteſt manner, to the phenomena now under conſideration; and it is even more forcible in the caſe of ſome of them,—in vaporization, for example. Here we know that a due ſupply of heat will cauſe an ounce of water to burſt a bomb-ſhell. Can any perſon aſcribe this to the *capacity* of the ſteam? This capacity is not yet acquired, nor *will* it be again acquired, unleſs the due ſupply of heat be at hand. The particles of water acquire the vaporous arrangement which renders them ſo capacious, *only in conſequence of the mutual action between them and the matter of heat.* In ſhort, it is plain that the abſorption of heat is not the *conſequence*, but the *cauſe* of the enlarged capacity. Strictly ſpeaking, they are concomitant events, and it is the *combining force* that is the immediate and operative cauſe of both.

Thoſe who aſſign the enlargement of capacity as the cauſe of the abſorption of heat, in a ſtate which does not affect the thermometer, are by no means preciſe and uniform in the meaning which they affix to the term. They muſt be ſenſible that mere *capaciouſneſs*, or *room*, will not apply to the phenomenon of melting ice, in which the room is undoubtedly diminiſhed in the liquefaction, and even for ſome time after. For, while the water, juſt formed, continues to accumulate heat around its particles, the bulk does not increaſe; on the contrary, it diminiſhes. In their conception of capacity is included the notion of a *diſpoſition* to abſorb heat; that is, of an *attraction* for heat; or, to expreſs it more philoſophically, the notion of the exiſtence of a force, which unites the matter of heat with that of the ice. What is this but the union or combination which Dr. Black at laſt contended for? Chemiſtry preſents a conſiderable variety of ſuch combinations, and there are many, eſpecially of the gaſes, which are as little permanent, and yield as readily to a diſ-

ference in the proportion of the ingredients, as thoſe combinations of bodies with heat. Nay, we ſhall find ſtill another combination of heat with other matter, which has all the permanency that can be deſired, and which the moſt pertinacious partiſan of capacity muſt admit as a true chemical union. I mean the gaſeous form. Oxygen gas is ſuppoſed to conſiſt of the baſis of vital air, united with the ſubſtances of heat and of light. It requires a certain temperature, and the preſence of a proper third body, to produce a decompoſition; and here we find a prodigious quantity of heat accumulated. Capacity will explain *this* accumulation, in the very ſame manner that it explains all the others.

I am diſpoſed to think that there is a gradation of union between the particles of matter and heat, which very much reſembles the gradation which late obſervations have diſcovered in the combinations of oxygen. The degrees of oxydation, as it is called, are as progreſſive, and as diſtinct, as thoſe exhibited in the effects of heat; and they are all produced by the gradual introduction or abſtraction of oxygen. There ſeems to be, firſt of all, a union ſimilar to that of mere adheſion. The heat thus united has been called free, moveable heat. Perhaps the union accompanied by expanſion, or which produces expanſion, is ſomething more cloſe, and it may be that only the ſuperfluity is free and moveable. This mode of union is evidently independent of the others, and may be ſuperadded to them all. Then comes the union which is characteriſed by fluidity. This is followed by the union of vaporiſation, and this by the union which forms a gas. Perhaps a juſt conception of theſe changes would be more eaſily ſuggeſted, were we always to employ the word *temperature* for the degree of the ſcale, and *heat* for the matter combined; and to call the heat concealed or latent, in a liquid, and in vapour, the *melting heat*, and the *boiling heat*; thus diſtinguiſhing them from the *melting temperature*, and *boiling temperature*.

In ſhort, this diſpute ſeems merely occupied about the propriety of a term: And even in this humble ſenſe, Dr. Black's employment of the term combination is more proper than the employment actually made of the term capacity. About the propriety of the term *latent heat*, as the expreſſion of a mere fact, there can be no diſpute.

In Dr. Black's examination of Dr. Cleghorn's method of explaining the phenomena of fluidity and vapour, he was obliged to conſider the uniting forces, not as a chemiſt, but as a mechanician. He diſliked this occupation exceedingly; and the remarks on Dr. Cleghorn's hypotheſis are almoſt the only examples to be found in the manuſcript from which he lectured. But obſerving this way of conſidering chemical ſubjects to be gaining ground, after having been ſo long and ſo profitably baniſhed from chemiſtry, he was induced to ſay more on the ſubject, in ſome particular courſes. Some notes of thoſe occaſional lectures remain. I recollect a converſation, in which he explained to me ſome of his notions of this kind. He ſaid that this way of conſidering the phenomena was only miſpending of time, becauſe it produced no clear conception, and gave no confident knowledge. He ſaid that he ſometimes thought himſelf ſucceſsful; but ſome other views convinced him that he had only been ſurrounding, in imagination, the particles with ma-

terial, active atmoſpheres, which, notwithſtanding their activity, and the mutual repellency of their particles, muſt be conceived to penetrate each other, without derangement of their forms; in ſhort, without the very activity which he had aſſigned them. He could recollect his being affected in the ſame way by reading Dr. Gowen Knight's ingenious eſſay on magnetiſm. The firſt ſteps of the inveſtigation ſeemed to give very clear notions; but he found them afterwards involved in continual inconſiſtencies. He had obſerved the ſame thing in all attempts of this kind; and he therefore avoided them. He was convinced of a ſort of rivalſhip between coheſive attraction and many chemical attractions, by which they counteracted and weakened each other. But this was only a way of conceiving, or rather of expreſſing, a plain fact; for when he conſidered them purely as attractions, there could be no oppoſition between them.

This is a very juſt account of the matter. Indeed, when we recollect that the united efforts of all the mathematicians of Europe have not yet given a demonſtrable and accurate account of the motion of three particles attracting each other, we can expect little ſucceſs when we want to determine the motion of millions attracting at once.

But, although we cannot perhaps explain phenomena by this way of conſidering them, we can diſcover, in ſome caſes, the inſufficiency of explanations which are in vogue. Nothing is more common than the notion that fluidity is produced by heat, merely by its deſtroying the attraction of coheſion. Obſerving that all the corpuſcular actions are weakened by an increaſe of diſtance, and that heat expands bodies, we think it plain that the expanſion may be continued till the particles get beyond the ſphere of coheſion, and that fluidity is the neceſſary conſequence. Dr. Black very properly obſerves, that the attraction of coheſion is not deſtroyed; becauſe we ſee that a fluid gathers into drops. He grants that it is weakened, and ſeems to think that the attraction, weakened to a certain degree, is the conſtitution proper to fluidity. But this conceſſion is not neceſſary, and this weakened attraction is not enough for fluidity.

It is by no means certain that the attraction of two particles of water is leſs than that of the ſame particles in the form of ice. The only effect of this attraction which can affect their motion, is their being preſſed together by it, as a piece of iron is preſſed to the magnet. Now, we know that water is equally fluid at the bottom of the ocean as at the ſurface. At the bottom, the particles are preſſed with a very great force, probably equal, nay, certainly ſuperior, to the coheſion of ice. The eaſy ſeparation of the particles of water is owing to this perfect mobility from one ſituation among the others to another ſituation. In ſhifting them from their places, we are ſcarcely oppoſed by any thing beſides their inertia. We act but on one, or but on a very few at a time. It is quite different when we attempt to ſeparate the parts of a ſolid. But, to underſtand this difference, we muſt think a little more of the nature of fluidity and ſolidity. I do not mean to enter into the laws of attraction, &c. but merely to reflect on what circumſtances are neceſſary for rendering an aſſemblage of matter a fluid, ſuch as we obſerve water to be. A fluid is an aſſemblage of matter, the particles of which yield to the *ſmalleſt* impreſſion,

and, by thus yielding, are moved among themſelves with the greateſt facility. A body, having a baſe perfectly flat and ſmooth, and lying on a horizontal plane, having no friction, can be puſhed to any ſide, in the ſame manner as if it hung in the air, whatever its weight and preſſure may be, becauſe it muſt be indifferent as to any ſituation on that plane. In like manner, in order that a particle of a fluid may be ſo eaſily moved from its place among the reſt, all that is neceſſary is, that all new ſituations ſhall be perfectly indifferent. That this may be the caſe, the action of a particle muſt be equal in every direction at the ſame diſtance; no matter how ſtrong this may be, or whether it attract or repel. It is quite plain that if this action be thus equal in every direction, it can require no force to remove an adjoining particle from one ſituation to another. Nor will it require any force to keep the particle in this new ſituation, becauſe this ſituation is indifferent to it, there being no inequality of action urging it to either ſide. The attraction and repulſion may vary according to any law, by a change of diſtance from the particle; but it is equal in every direction at that diſtance. An aſſemblage of ſuch particles muſt have all the well known appearances of a fluid. It will gather into drops perfectly ſpherical, becauſe any other form would cauſe an inequality of the compound action on any one particle, to which inequality of action the particle would yield, and it would ſhift its place. Such an aſſemblage will, if heavy, acquire a horizontal ſurface, &c. &c. It may be demonſtrated that ſuch a particle muſt either be a ſingle atom of matter, or muſt be a ſphere containing numberleſs atoms *ſymmetrically* diſpoſed. The ſpherical form is neceſſary for fluidity, not for the reaſon commonly aſſigned, that they may touch in few points, and eaſily ſlide on each other. Theſe would not conſtitute a fluid; for a parcel of ſpheres, perfectly ſmooth, could be piled up in a pyramid. It is neceſſary merely in order to produce this equality of action in every direction.

Solidity on the other hand, requires a very different law of action among the particles. All ſituations muſt *not* be indifferent. A particle muſt affect a *particular ſituation* among the adjoining particles, muſt require ſome force to change its preſent ſituation, and when this is changed a little, it muſt have a tendency to regain it, and muſt actually regain it when the diſturbing force is removed.

To effect this, it is only neceſſary that a particle attract or repel another more ſtrongly in one direction than in another, at the ſame diſtance; and if the action of a particle be thus unequal, an aſſemblage of ſuch particles cannot be fluid. The particles will unite in one *attitude* rather than in another. Force is neceſſary for changing this arrangement; and if this change be very ſmall, it will be reſumed when the force is removed.

All this will be illuſtrated by attending to the appearances exhibited by a parcel of ſmall bits of iron wire, (ſuppoſe one-tenth of an inch long) ſcattered at random on the ſurface of quickſilver. They will lie in any poſition, and the touch of a ſingle hair will change their poſition at pleaſure. But now bring a great and powerful magnet near them, they will all inſtantly change their reſpective attitudes and poſitions. Each will turn its ends towards the ends of the adjoining particles,—they will run together into

certain curious groupes. The mere touch of a hair will not change theſe new poſitions and attitudes; or, if it ſhould change them a little, they will reſume them when the hair is withdrawn. The reaſon of all this is merely this, that the neighbourhood of the magnet makes them all little magnets, and now each little bit attracts *more ſtrongly* by its ends than by any other part, and in one direction much more than in another, ſo that there is a particular poſition and attitude, which each will aſſume and maintain with a certain minute force. Remove the magnet, and they all return to their former ſtate of indifference and mobility.

The conſtitution of a ſolid body reſembles this. The particles have ſuch an inequality of action, reſembling the polarity of magnets. It is by this, operating in the moſt favourable circumſtances, that moſt ſolid bodies acquire a ſymmetrical arrangement of parts. Thus do ſalts concrete from their ſolutions in a cryſtalline form.

Here then we have a *narration of facts* which cannot be diſputed. A fluid *muſt* conſiſt of particles, which, while they attract each other, will take any ſituation indifferently. A ſolid conſiſts of particles which have a quality reſembling the polarity of magnets; and we underſtand in ſome ſmall degree, on what dynamical principles theſe conſtitutions depend.

Now, let heat be applied to a ſolid body, and let it continue to expand it till its coheſive attraction is deſtroyed, what is the effect? It now is an aſſemblage of incoherent particles. But this is not a fluid; it is a duſt, an impalpable powder, which may be heaped up like ſo much flour; or it is a parcel of ſlightly *adhering* particles like wet ſand, or half-boiled barley. But it is not fluid. Heat muſt do a great deal more to make it a fluid. It muſt render each particle equally active in every direction, deſtroying its former polarity.

Thus, we ſee clearly that the particles do not firſt fall aſunder, then take the arrangement of fluidity, with an enlarged capacity; and laſtly, *admit* the heat into the enlarged pores. The arrangement of fluidity requires the previous change of the law of action, and this muſt be produced by the heat. Therefore Dr. Black was fully warranted to ſay that fluidity was immediately effected by heat.

What has been ſaid of fluidity is alſo true with reſpect to the vaporous form of aggregation, and need not be repeated.

We know very little of the corpuſcular conſtitution of bodies: Yet we know ſomething, and can make ſome uſeful inferences. We know that all the corpuſcular forces diminiſh faſt by an increaſe of diſtance; and *we can demonſtrate*, that the *inequality* of action on which ſolidity depends, decreaſes much faſter. We ſee this remarkably in magnets. At a ſmall diſtance the action of a magnet, in different directions, is very unequal; but as we recede, the inequality of action diminiſhes exceedingly. We can demonſtrate ſtrictly, that if a body, acting in this unequal manner, be ſurrounded by an aſſemblage of ſmaller particles, which act equally at the ſame diſtance at leaſt, the inequalities in the action of the nucleus will be prodigiouſly diminiſhed by the ſuperadded equable action of

this ſort of atmoſphere. Nay, we can ſhew that this atmoſphere may be ſo diſpoſed around the particle, as to take away entirely the polarity or inequality of action. We ſee, therefore, that the accumulation of heat round a ſolid particle, muſt, independent of the expanſion cauſed by it, diminiſh the coheſion of ſolidity, and cauſe the union to approach, at leaſt, to the nature of the union of fluidity. The great quantity of heat that is accumulated round the particle in the liquefaction gives ſtill more weight to this argument. We ſee too how the abſtraction of heat, and the contraction of bulk, tend to bring the unequally acting parts nearer to each other, and to give occaſion for the coheſion of ſolidity. When a maſs of water has cooled below the freezing temperature, nothing induces the freezing with ſo much certainty as the touching of it with the point of one of the fine ſpiculæ which have been formed by freezing. It is evident that the polarity of this ſpicula, by its action on a particle ſurrounded by a ſmall quantity of heat, muſt diſpoſe the heat to emerge in fine ſtreams from certain points of it, by deſtroying that equilibrium of heat around it which fitted the particle for the fluid aggregation. I am far from offering this as a complete explanation; but I ſay that if heat conſiſts of particles much finer than thoſe of tangible matter, and if it be capable of being conſtipated round them, it cannot but produce effects of *this very kind*, and that it is poſſible for it ſo to accumulate round them, as completely to effect the changes that we obſerve.

The change from the liquid to the vaporous form is much more eaſily conceived, but does not afford ſuch cogent proofs of the important part which is acted by heat in producing the change. But there is ſomething curious in the law of expanſion that ſeems to obtain in vapours. Equal augmentations of temperature (which ſeem to ariſe from equal additions of the matter of heat) produce nearly equal *multiplications* of bulk. This ſeems to indicate an equal denſity of heat in all temperatures. For, if the atmoſphere of heat be like other elaſtic atmoſpheres, equal additions of the matter of heat ſhould produce additions of bulk, which continually increaſe.

There is another circumſtance in the vaporous combination of heat, which I think of immenſe importance to the philoſophy of chemiſtry, namely, its being counteracted by mechanical force. Not only can the elaſticity or mutual repellency be counteracted by preſſure, but *the chemical union of heat and the matter of a body can be prevented by it.* The inſtant that the preſſure on the ſurface of boiling water is removed, the union takes place; for ſteam is produced, and the temperature of the water is diminiſhed. Hold the ball A of the pulſe-glaſs repreſented by fig. 3. in the mouth. The liquor will boil in the ball B, and A will feel cold. Graſp B in the warm hand, and the ebullition is then ſtopped by the elaſticity of the ſteam, which is allowed to retain this form by the warmth of the hand. A immediately grows warm alſo. Now remove the hand from B, and let it be wetted with ſome ſtrong ſpirits of wine. This produces a cold which condenſes the ſteam in B, removes its preſſure, and the ebullition begins again. At the very ſame inſtant A feels vaſtly cold. Thus was the combination of water and heat in A effectually prevented, although the mutually acting ſubſtances are contiguous as before. Add to

all this, that when the vapour is produced by the union of heat with tangible matter, preſſure will put an end to it, the vapour becomes water, and the heat flies off. We cannot form a conception of any thing oppoſing a mechanical force, but another mechanical force. Hence we muſt infer that the corpuſcular powers or qualities which ultimately produce the chemical phenomena, are not different from the forces which produce and modify local motion, and are really ſimilar to thoſe which produce the fall of heavy bodies, and the communication of motion by impulſe. There were ſtrong reaſons for thinking ſo, independent of this fact. Capillary attraction, adheſion, coheſion, and ſome of the effects of electricity and of light, furniſhed ſurmiſes of this kind. But the prevention of vaporiſation by external preſſure is the moſt direct proof. The conjecture of Sir Iſaac Newton, expreſſed in one of his reflections at the end of his treatiſe on Optics, ſeems fully verified; and it becomes highly probable that all bodies conſiſt of atoms endowed with one and the ſame power of attraction and repulſion, varying according to a determined law of the diſtances. The ingenious eſſay on this ſubject by the Abbe Boſcovich merits therefore the careful peruſal of every philoſophical chemiſt.

[*Note* 6. *p.* 205.]

There is ſomething very curious and unaccountable in the manner in which theſe vapours are precipitated. It is not in round drops, as one ſhould naturally expect. Were this the caſe, the precipitation would be almoſt inſtantaneous. A drop, whoſe diameter is the thouſandth part of an inch, would acquire the velocity of nine or ten feet per ſecond; whereas we ſee clouds hover at a very ſmall elevation, for many hours, and they can thus be tranſported from the ſea, lake, river, or marſh, from which they are raiſed, far into the country, where ſuch ſupply cannot be had any other way. Thus is water carried to the tops of the mountains, where it is condenſed, ſupporting their vegetation, and giving the ſurplus to the ground, there to percolate and ſupply us with ſprings and ſtreams of freſh water. A cloud formed of ſolid drops would always exhibit a rainbow when oppoſite to the ſun; and when between us and the ſun, would exhibit coloured rings altogether unknown in the ordinary courſe of things.

The precipitated or falling miſt is all in the form of thin tranſparent bladders or veſicles. This was affirmed by Derham and others, but without any authority, or indeed any aſſertion that they had obſerved them. Mr. Sauſſure, in a journey of obſervation among the Alps, found himſelf in a thick miſt, which was almoſt ſtagnant. He was aſtoniſhed at the ſize of the drops, as he thought them, and at ſeeing them ſwimming ſlowly by him without falling to the ground. Some of them were larger than the largeſt peaſe. Catching them in his hand, he found them to be nothing but bladders inconceivably thin. He now found them all to be of this ſtructure. He is the firſt perſon, ſo far as I know, who has deſcribed them. I have many years ago obſerved that the particles of miſt viſible in the receiver of an air-pump, are bladders. I inferred this from their

ſize, and from their very ſlow deſcent through air ſo much rarefied. I was much ſurpriſed at them, but imagined that this was peculiar to the precipitate formed by ſudden rarefaction. But I now ſee that Mr. Sauſſure's deſcription is general. The miſt of boiling water, when between the eye and a candle, and between us and the ſun, exhibits the halos which are producible by veſicles only. This is eaſily demonſtrated by the laws of optics.

This conſtitution of a miſt or fog, ſo neceſſary for accompliſhing the great purpoſes of nature, is to me moſt unaccountable. I cannot conceive how the formation of ſuch a veſicle commences, or what is the aëreal ſubſtance which diſtends it. It is an air in a ſtate of ſlight compreſſion; for the film of water round it is endeavouring to contract. I ſee ſuch bubbles ſometimes ariſe from lintſeed oil, when boiling to free it from its watery part. This I underſtand, but its origin has no reſemblance to the production of a miſt. The veſicles may be very diſtinctly ſeen in the evaporation from a diſh of very warm coffee without milk. Set it in the ſun, and, in viewing it, turn the back to the ſun. When thus viewed through a magnifying glaſs, the veſicles are ſeen riſing in great abundance; ſome of them ſo large and heavy, that they fall down immediately, and roll about like ſo much dry duſt on the ſurface of the coffee, appearing like brilliant ſpots on the dark ground. *(See Sauſſure's Eſſai ſur l'Hydrometrie, page* 285.*)*

[*Note* 7. *p.* 210.]

If heat be the ſole agent in producing the vapour, I cannot ſee how the air above the evaporating body can be uniformly tranſparent. Although the vapour may have the ſame elaſticity with the air, it is very improbable that it has either the ſame weight, or the ſame refracting power, and it muſt always ſurround the evaporating body with an unequable atmoſphere, like a mixture of oil and water ſhaken together. The neceſſity of renewal of air, the ſtop to evaporation by ſtagnation, and even the effect of extended ſurface, are all inexplicable, if heat alone be the agent. Theſe facts are ſtrong arguments for believing that the air *alſo* acts by its diſſolving power. The prevalence of bad ſmells immediately before rain, is a fact very ſimilar to the precipitation of one ſubſtance by means of another. Dr. Hutton's obſervation, that two maſſes of very damp air, of very different temperatures, precipitate water on mixture, is a fact ſimilar to a like precipitation of nitre from two ſaturated ſolutions of different temperatures, but is inexplicable by the other theory. Dr. Black thought very highly of Mr. Sauſſure's eſſais ſur l'hydrometrie, and of his general method of philoſophiſing, which is perſpicuous, logical, and modeſt.

[*Note* 8. *p.* 216.]

I do not think that I can conclude this article as I ought, without taking notice of ſome facts and circumſtances, which affect Dr. Black's claim to the being conſidered as

the diſcoverer of the peculiar combination of heat, or the cauſe of heat, with different ſubſtances, by which they are made to exiſt in the form of a liquid or of an expanſive vapour. His title to this diſcovery, and to the theoretical doctrine founded on it, has been called in queſtion. I am not certain that it is generally acknowledged on the continent, or that it is even generally known that he has occupied himſelf with this reſearch. I ſee it treated in the compilations of many of the foreign chemiſts, with more or leſs ability, as the diſcovery of Mr. Wilcke of Stockholm, and by others as coming from Mr. De Luc, and by many as a part of the great diſcoveries of Mr. Lavoiſier. I find this laſt opinion very general, and I ſee clearly whence this ariſes. Mr. Lavoiſier and Mr De la Place publiſhed the firſt of their valuable diſſertations on the relations and meaſures of heat in 1782. Of the doctrines contained in theſe diſſertations, this doctrine of *combined* (or, as Dr. Black calls it *latent*) heat, is the baſis, and it is involved in all the chief propoſitions eſtabliſhed by theſe philoſophers. Yet the name of Dr. Black is not mentioned. Theſe diſſertations ſeem to be received as the original ſource of the whole doctrine, and to be conſidered as the reſult of *their* labours alone.

Although Dr. Black was duly conſcious of his own merit as a chemiſt and a philoſopher, there never was a man leſs aſſuming, or leſs eager to obtrude himſelf on the public attention. Even his diſcovery of fixed air, and his ingenious theory of quick-lime, would not perhaps have been committed to the preſs, had they not formed the ſubject of his inaugural diſſertation, when he received his medical degree. His doctrine of latent heat, and the theory of its combination with all kinds of matter, in the forms of liquid and of vaporous aggregation, although incomparably more ingenious, and of more extenſive and important application to the explanation of natural phenomena, were known to the world only through the lectures delivered to his own numerous ſtudents. Scarcely did a year paſs over, after his ſettlement in Edinburgh, in which ſome corner of this doctrine was not nibbled off, and appropriated by others; generally by his former pupils. He was often urged in the moſt preſſing manner by his friends to publiſh, and was told that the whole of it would, in this way, be ſoon honeſtly portioned out among different perſons, as the fair reſult of their own inquiries; and that if he ſhould at laſt publiſh, he would only be conſidered as a copyiſt. Yet nothing could prevail on him (ſo much did he diſlike authorſhip) to vindicate his claims, till he learned that his friend Mr. Watt was likely to ſuffer by his ſilence, becauſe others were handing about as their own ſome very peculiar views which he had taken of the ſubject, dependant on Dr. Black's doctrine. The Doctor was ſo far rouſed by this, that he promiſed to Mr. Watt that he would print an account of theſe ſpeculations without loſs of time. But this went no further than writing two octavo pages, and noting with his pencil ſome particulars for continuing this account.

In the mean time, others were appropriating to themſelves various parts of the doctrine of latent heat, or, without perceiving the connection of the whole, were acquiring reputation by ingenious explanations of particular phenomena, which depended on this doctrine. Thus, in or about the year 1772, Profeſſor Wilcke of Stockholm read in the

Royal Society in that city, a diſſertation, in which he ſhewed that ice abſorbed, whiſe melting, 72 degrees (by the Swediſh ſcale) of heat. Magellan publiſhed a work, in which he aſſerts, in a very authoritative manner, that Wilcke is the author of the diſcovery. Others repreſent Dr. Cullen as the author, becauſe his ſurpriſing experiments with æther *in vacuo*, exhibit the moſt remarkable example of the abſorption of heat. But Dr. Cullen never laid the ſmalleſt claim to the doctrine. Many affirm that Dr. Irvine and Dr. Crawford ſhare at leaſt the honours of the diſcovery with Dr. Black, and that they had more juſt conceptions of the phenomenon than their preceptor.

The claims of theſe gentlemen are, however, refuſed by that eminent naturaliſt Mr. De Luc, in his *Meteorologie*, publiſhed in 1786. But this refuſal is not in order to ſecure the diſcovery to its worthy and unambitious author. Mr. De Luc condeſcends indeed to ſay that *Dr. Black was the firſt who* ATTEMPTED *to meaſure the quantity of heat that is abſorbed during the melting of ice and the boiling of water.* But he conſiders himſelf as the firſt author of the doctrine that *heat combines with the particles of a ſolid body, and that the reſult of this combination is the liquefaction of the ſolid, and its exiſtence in a fluid form; heat or fire being one of its conſtituent parts; and that, in like manner, another combination of fire with the particles of bodies converts them into elaſtic vapour, of which fire is a conſtituent part.* He refers the reader for proofs to his publication in 1772, under the title of *Recherches ſur les modifications de l'atmoſphere*, of which this theory, modified in a very particular manner, makes a principal part. He profeſſes to have formed this theory in the years 1754, 1755, and 1756, and quotes ſome obſervations or experiments, of that date, on which he affirms it to be founded. He conſiders Dr. Black's doctrine of latent heat as making but a part of his own theory, which extends to every caſe of evaporation, while Dr. Black's relates only to the heat abſorbed in ebullition, and combined in elaſtic ſteam.

Mr. Watt, warmly attached to his highly reſpected friend Dr. Black, was ſurpriſed at this faint and inadequate conceſſion of Mr. De Luc, and was grieved that the Doctor's inactivity had given room for it. Knowing that every publication of ſo eminent a philoſopher as Mr. De Luc would ſtrongly impreſs the public mind, and would leave it uninformed of what Dr. Black had done, he wrote to Mr. De Luc a letter, in which he aſſerts that Dr. Black had done much more than *merely* TRY *to meaſure the quantity of heat abſorbed, &c.; that he had firſt diſcovered and demonſtrated that heat is abſorbed, and is combined with bodies when they are rendered fluid or vaporous, &c.*; and he inſiſted that this letter ſhould be inſerted at the end of the next volume of the *Meteorologie*. Accordingly it is there inſerted, in page 503 of the appendix. Mr. De Luc expreſſes his regret that his words had been taken in a ſenſe different from that in which he had employed them, and profeſſes the higheſt regard for Dr. Black. But he ſtill retains his claim to diſcovery and to priority.

There is nothing of which I am more fully convinced than that Dr. Black is the *ſole author* of the doctrine of latent heat; and, when the following particulars, leading to

that conviction, which my duty to my much reſpected teacher and friend impels me to lay before the reader, are duly conſidered, it will remain with him to ſay whether he coincides with me in the belief, that whatever portions of this doctrine may have been ſeen, or gueſſed at, on the continent, or in theſe kingdoms, they have emanated originally from Dr. Black, probably through the medium of ſtudents attending his lectures, as they did from every part of Europe.

Dr. Black was ſo little ſolicitous to ſecure his title to diſcovery, that I got very little information to this purpoſe among the notes from which he lectured. But, ſearching among his old memorandums and diſcarded papers, I found ſeveral ſmall note books of an uncommon form, ſuch as I remembered to be much uſed by the ſtudents in the Univerſity of Glaſgow, and were ſold in the ſhop of the Univerſity printer. Theſe little note books were filled with memorandums of things which had engaged the attention of the young philoſopher; and theſe were mixed with notes of occurrences, and names of perſons, which were all familiarly known to me, I being a native of the place. By theſe circumſtances, the dates of many memorandums may be aſcertained within a month; and they ſhew the whole to have been written in 1754-5-6-7, while the Doctor was a ſtudent at Glaſgow and Edinburgh, and during the firſt year of his Profeſſorſhip in Glaſgow. I ſhall inſert a few of ſuch notes as regard the preſent ſubject.

No. II. Page 1. " Does not the heat produced in ſlaking lime, and in the ſolution " of lime and cauſtic alkali in acids, ariſe from the ſudden fixing of the fluid part in a " firm ſtate? taking them from a ſtate of fluidity.

Page 2. " Intenſe cold produced by ice diſſolved in an acid explained, and the prin- " ciple applied to cryſtallization. Is not ice cryſtallized water? Is it not ſimilar to a ſolu- " tion of a ſaline cryſtal? A greater alteration of heat ſhould be produced by mixing " a neutral ſalt with ice or ſnow, than by diſſolving it in water.

No. III. Page 1. " One way of eſtimating the quantity of heat which is transferred " and diſappears in the production of vapour, or which is expended, and contributes to " the formation of vapour, but cannot be diſcovered in the vapour by the thermometer.

" Place a phial with water, cloſe corked, in a ſtove or oven, where it will acquire " a degree of heat above the boiling point: Then, by ſome contrivance, open it ſud- " denly, and obſerve how much has been converted into vapour, and what heat it " retains."

N. B. In the next page Dr. Black deſcribes the method for examining the thermometric ſcale, ſo as to decide whether the ſcale of equal expanſions correſponds to equal augmentations of heat. Mr. De Luc ſeems to conſider himſelf as the firſt who examined this queſtion alſo.

No. IV. Page 6. " Nitrous æther boils *in vacuo*, and grows vaſtly cold. Is not the " cold produced merely by the evaporation of the elaſtic parts; or does it emit air, which " requires heat for its elaſtic appearance?

Page 20. " In the effervescence of mercury in nitrous acid, whence comes the great heat

" which muſt certainly be required; for ſo much elaſtic matter muſt rob it, as æther " boiling *in vacuo* does?

" All ſolutions muſt be conſidered as liquefactions in conſequence of heat, for there " is no fluidity without heat. Salt and water, when mixed, melt with much leſs heat " than what will melt ice, and vaſtly leſs than what will melt ſalt.

Page 30. " Does it not appear, on a review of cooling mixtures, that a refrigeration will " always happen, when ſalt, in a cryſtalline form, (that is, combined with water) or " water in a cryſtalline form, (that is, frozen) is made fluid by ſalts, or in ſpirit of wine. " According to this ſuppoſition, ſalt mixed with ice ſhould produce much more cold " than ſalt mixed with water that is as cold as the ice.

" Will not the progreſs of cooling be retarded while a ſolution of ſalt cryſtallizes? " Try if this happens while fats become ſolid.

" Do not theſe ſalts produce the greateſt cold which are diſſolved by the ſmalleſt quan- " tity of water?

Page 32. " Will a boiling fluid raiſe the thermometer in proportion to the preſſure on " its ſurface? Will this be of any uſe in diſcovering the abſolute proportions of heat?

No. V. " Much greater heat when a cauſtic ley is mixed with an acid, than when a " mild one is; none is taken off by the elaſtic effervescence.

Page 15. " Will diſtillation *in vacuo* ſave fuel?

Page 16. " Is not heat produced in ſolutions, when a fluid matter is converted into a " ſolid? Calcined Glauber ſalt and water; but not when ſolid matter is rendered fluid.

Page 24. " Is not the cold produced by mixing vitriolic acid with ſal ammoniac owing " to the production of elaſtic matter?

No. VI. " Is the heat which is diſengaged from water in becoming ſolid in a cryſtal, " equal to what muſt be employed to disjoin it from the water? How may this be tried? " In a ſalt it is not all diſengaged, for the brine is ſtill ſalt; and when it efflores̄ces, " more muſt be employed to produce vapour."

By theſe memorandums, I think that it muſt be evident to any reader, that the doctrine of latent heat, as a neceſſary ingredient in all fluids and elaſtic vapours, has not been an opinion ſuddenly formed by Dr. Black, or deduced from a ſingle phenomenon. Nor has it ariſen from a general, but vague view of all the phenomena of liquefaction and vaporiſation. It has long occupied the mind of the ingenious author, and his ſurmiſes have been noted down, that they might not be forgotten, while preſent ſtudies, or the circumſtances of his ſituation, hindered him from putting his conjectures to the teſt of experiment. He might truly ſay, therefore, as in page 156, that he ſcarcely remembers the time when ſome ſuſpicion of this combination did not poſſeſs his mind. The theory has been the *matured* reſult of many obſervations, and frequent meditation. The reader will not wonder that Dr. Black's mind was not ſooner made up upon this ſubject, when he recollects that he was elected Profeſſor of Medicine, and that chemiſtry was only a lectureſhip. His medical ſtudies muſt have occupied moſt of his time; and the

attention which he gave to his patients was ſo anxious and unremitting, that one ſhould have thought that he had not an hour left for any other duty. Mr. Watt thinks that he had completed his proofs of the abſorption and emerſion of heat in liquefaction and congelation in 1758. But, in an old and rejected ſheet of lectures, which ſome other circumſtances oblige me to refer to this year, he treats the ſubject ſtill as a general view, and makes no mention of the *quantity* of heat which enters into the formation of water. I think that this would not have been omitted, had the experiments been already made. I am perſuaded, however, that the extenſion of the doctrine to the ebullition of fluids followed very ſoon after its eſtabliſhment in the caſes of liquefaction and congelation. The fundamental phenomena, and the experiments for determining this part of the theory, appear to me to be much more obvious and impreſſive than in the other caſe. I am ſo much of this opinion *now*, that I wonder, as often as I think of it, that the heat latent in ſteam was not detected even ages ago. At all events, it is fact, that the doctrine of latent heat, preciſely as it is contained in theſe lectures, was explained and demonſtrated by Dr. Black in 1761, and in every ſubſequent courſe.

I muſt now take the liberty of ſaying that I was particularly hurt, when I was informed of the cold, and, as it were, reluctant conceſſion of Mr. De Luc, " *that Dr. Black* " *was the firſt who had* ATTEMPTED *to meaſure the quantity of heat which ice abſorbs during* " *its liquefaction,*" &c. becauſe I had long been made to expect from Mr. De Luc a very different account indeed of Dr. Black's labours. At this time (1786), I had been for ſeveral years the colleague of my former preceptor, and I took the liberty to join in the importunities of his friends, who urged him to print an account of his doctrines. His health was very delicate about this time, and we deſpaired of his being able to undertake the taſk. We were conſoled, in ſome degree, by hearing that Mr. De Luc had *repeatedly* offered to take this trouble on himſelf. We were given to underſtand that, three or four years before this, the Doctor had been prevailed on to communicate the neceſſary materials to that gentleman for this purpoſe, and that the account would ſpeedily appear, in a great work which he was about to publiſh; and it was ſaid that Mr. De Luc was particularly deſirous to have this opportunity of maintaining Dr. Black's right, in oppoſition to the pretenſions of Dr. Crawford and others, who had endeavoured to rob him of his deſerved fame.

The account which did at laſt appear was indeed a matter of ſurpriſe and diſappointment: For it was underſtood that Mr. Watt had been allowed to communicate to Mr. De Luc the leading points of his theory, and even to repeat with him the experiments neceſſary for its demonſtration. Yet not a word is ſaid by Mr. De Luc of the attempts of Wilcke, Dr. Crawford, and Dr. Irvine, to explain the phenomena in another way, except in order to refute them by his *own* NEW *theory of watery vapours*. Nay, the explanation which Mr. De Luc vouchſafes to give, after inſerting Mr. Watt's letter, and acknowledging that his words were ſuſceptible of the interpretation given of them, is merely condeſcending to ſay that *it was a great ſatisfaction to him to learn, by the letter of Dr.*

Black's friend, Mr. Watt, that his own ſyſtem of the combinations of fire, as a conſtituent part of certain ſubſtances, had had Dr. Black for a defender at that time, as it has ſince had in Mr. Lavoiſier; and that he will have a greater confidence now in his opinion, ſince it is common to him with naturaliſts of their rank.

This is really too much. Had Mr. De Luc forgotten his repeated offers and requeſts to become Dr. Black's editor, and his importunity to be inſtructed in the particulars? Had he forgotten his being a witneſs, in 1782, of ſome of the principal experiments on the latent heat of fluids, when boiling under the preſſure of the atmoſphere, and alſo *in vacuo*, and the important deduction made from them by Mr. Watt?

Let it not be thought that I make theſe aſſertions reſpecting Mr. De Luc's acquaintance with Dr. Black's doctrine, on the ſole authority of what I heard in converſation with Dr. Black's friends. I have now lying before me the unfiniſhed ſcroll of a long letter of Dr. Black's handwriting to Mr. Watt, in which every particular that I have mentioned is alluded to. Mr. De Luc's repeated ſolicitation to be his editor,—his intention of doing him juſtice, by ſupporting his claims againſt the encroachments made on it,—aſſurances from Mr. Watt that Mr. De Luc will be a zealous friend,—Dr. Black's expreſſions of confidence in Mr. De Luc's genius and honour,—ſpecification of the things to be communicated, and the experiment to be ſhewn him. The letter is dated January 30. 1783, but is not finiſhed.

Let us now conſider a little the authority on which Mr. De Luc founds his claim to the diſcovery of the abſorption and combination of heat or fire with the particles of other matter, ſo as to become a conſtituent part of every fluid and of every vapour.

For this the reader is referred to the *Recherches ſur les Modifications de l'Atmoſphere*, publiſhed in 1772. The authority referred to in that work for the latent heat of melted ice, is an obſervation ſaid to have been made in 1754, *that there was no riſe of temperature in a veſſel containing ice melting, till the whole was become fluid.* I believe that this has been noticed by every naturaliſt, ſince the time that Dr. Hooke recommended melting ſnow for ſettling a point in the ſcale of temperatures; for the propriety of this method for aſcertaining a determinate temperature reſts entirely on the truth of this obſervation. Both Fahrenheit and Mairan, after remarking that the temperature riſes to the melting degree, affirm that it remains fixed there till all be melted. But neither of theſe philoſophers infer from this fact that the ice is continually *abſorbing heat*, and that the heat ſo abſorbed may be meaſured by the time of the liquefaction. Neither does Mr. De Luc make any ſuch inference: *Nor does he afterwards employ this inference as a principle in explaining any phenomenon.* Nor does he ſhew how this abſorbed heat may be made to emerge again, and warm other bodies.

This inference, this concluſion drawn from the conſtancy of the temperature, was reſerved for Dr. Black; and he proved that it was a juſt inference, by ſhewing that the ſame quantity of heat is afterwards obtainable from the water, when it freezes. Mr. De Luc noticed the fact indeed, that, during the liquefaction, the temperature was conſtant: But

it took no hold of his mind. It ſeems to have been forgotten till 1772. I do not ſee therefore that he can found his opinion or his theory on this obſervation. It does not receive or admit any explanation from his ſingular theory of watery vapours.

How different is all this from the procedure of Dr. Black? The notes which I have inſerted ſhew that a certain opinion had long lurked in his mind concerning the conſtitution of fluids, and that from time to time it had gathered ſtrength, in conſequence of his obſerving claſſes of phenomena, which would be juſt what we ſee them, on the ſuppoſition that heat is a conſtituent part of all fluids, and that fluidity is the effect, and the only ſign of this particular combination of heat with other matter. The propoſed experiment with the corked phial is the one which was made long after by Mr. Watt, with Papin's digeſter. The conjecture about the intenſe cold in freezing mixtures is preciſely what was afterwards found to be juſt. In ſhort, Mr. De Luc's obſervation was juſt one of thoſe remarks that are frequently made by an ingenious man habituated to ſpeculation; but it lay for years unimportant and neglected, among other memorandums; whereas there was a prepoſſeſſion, or an impreſſion on the mind of Dr. Black, which directed his attention to certain claſſes of facts, and was nouriſhed and ſtrengthened by ſubſequent remarks and reflections, till a great body of evidence at laſt incited him to a ſerious diſcuſſion, which terminated in conviction.

I have already ſaid that the arguments for the vaporous combination of heat are much more obvious and impreſſive. I am not ſurpriſed, therefore, that ſo ingenious and inventive a mind as that of Mr. De Luc very early conceived this with ſome degree of confidence and preciſion; and yet, even on this head, Mr. De Luc is ſatisfied with *general obſervations*. In § 676, he ſays diſtinctly enough that "*Fire quits the fuel, to unite with* "*the water, converts it into a vapour, and eſcapes with it.*" Farther, in § 684, he generalizes the fact, by *aſcribing all evaporation to the union of fire with water*; and in § 693, he explains on this principle the cold produced by evaporation. But, as Mr. Watt very properly obſerves in his letter, Mr. De Luc has made no experiment to demonſtrate any part of this theory, or to determine how much heat is abſorbed or emitted. Surely, till ſomething like this be done, the whole theory is nothing more than an ingenious but vague conjecture, reſting entirely on general obſervations, and deſtitute of that preciſion that is required for the foundation of a theory. Muſchenbroeck had formed the ſame conjecture long before (See his *Natural Philoſophy*, tranſlated by Sigaud de la Fond § 1455, &c.) He ſays that *the fire enters the water, throws it into ebullition, and that the greateſt part of the igneous matter eſcapes, but a portion remains combined with the water, and gives the vapour its expanſive form and force.* As for the acceſſary circumſtances of Mr. De Luc's theory, and his manner of conceiving fire as an *expanſive fluid*, acting the part of a carrier to the water, and the mode of its efficiency in taking up or depoſiting the water with which it is combined, I admit that *this is the entire property of Mr. De Luc.* I content myſelf at preſent with agreeing with Mr. Watt, that no experiment is adduced by the author, by which any part of the theory can be ſaid to be demonſtrated. I have not the leaſt notion how this theory of vapour is to be connected with that of liquefaction.

In the mean time, I requeſt the reader to recollect that the *Recherches*, &c. were not publiſhed till 1772. The main queſtion is, whether Mr. De Luc held this doctrine in 1754 or 1756? I rather think that he did not. He only ſays (*Meteorologie, app. p.* 512.) that in his *Recherches* he made uſe of obſervations which he had made in 1754 and 1755, on the phenomena exhibited by freezing water and melting ice, and of thoſe made in 1756 on vapours, and he acknowledges that, with reſpect to the firſt, he went no farther at that time. Now, the proofs of abſorption, combination, and emiſſion, and the mode of reaſoning from thoſe proofs, are ſo ſimilar in the two caſes, that I cannot conceive how a naturaliſt who draws concluſions from the one does not find himſelf incited, nay obliged, to make the ſame inferences from the other. I preſume that Mr. De Luc, inceſſantly occupied with meteorological ſtudies, has been conſiderably later in forming the theory, explained *in ſome degree* in the reſearches, and has found that theſe old obſervations or remarks alſo were explained by it, and might therefore be adduced as proofs. But, can a perſon be thought to have any clear notions of the ſubject, who ſays in § 676, that "water extinguiſhes fire, becauſe it has an affinity with it, and that air on the contrary animates fire, becauſe it has no affinity with it?"—who ſays, that "by the preſſure of the air, the fire is confined in the combuſtible or the hot body, and kept in priſon, but, when this compreſſion is removed, the fire immediately darts out, and is diſſipated?" who ſays, § 684, "that the particles of fire, being in continual agitation, knock on the particles of bodies, ſeparate them, and in their eſcape from the body, carry off ſome watery particles with them, and thus become the vehicle of the water, and conſtitute what we call watery vapour?" This is in no reſpect different from the vague writing of Muſchenbroeck, who is always groping about, and catching at any thing that will bear an application to his ſubject, and then gives us a ſtrange mixture of mechaniſm and chemical affinity, which both mechanicians and chemiſts will only ſmile at. By collecting ſuch vague and occaſional obſervations of the ancients, Mr. Dutens found in their writings all the diſcoveries of the two laſt centuries, and Mr. De Luc may find in his *Recherches ſur les Modifications de l'Atmoſphere*, every diſcovery made ſince 1772. Such writing, by its want of preciſion, becomes a net which will catch both fiſhes and fry. Nothing can eſcape it. And ſhall this be compared with the clear and preciſe propoſitions of Dr. Black?—Theſe may be falſe, but we know exactly what they are, and can form the moſt diſtinct notions of them, ſo as to ſubject them to the teſt of experiment, which will at once eſtabliſh or refute them.

I have ſaid that the *Recherches* did not appear till 1772. Dr. Black ere this had given at leaſt ten or twelve annual courſes, in which his doctrine of latent heat was demonſtrated and explained. He had many pupils from Geneva, ſome of them very well informed young gentlemen, and very able to underſtand this doctrine. I remember a Mr. Chaillet at Glaſgow in 1763, and a Dr. Odier in Edinburgh, who (at leaſt the latter) correſponded with Mr. De Luc, and communicated ſeveral of his valuable obſervations to Dr. Black. But we never heard of ſuch opinions being held by any other perſon beſides our own profeſſor. I ſhould think it not unlikely, that a philoſopher of Mr. De

Luc's ardour and aſſiduity, who held a correſpondence with the learned in all parts of Europe, would be pretty ſoon informed of Dr. Black's ſpeculations. Indeed there was a ſurreptitious and imperfect publication made of them at London by *Nourſe* in 1770, entitled *Enquiry into the General Effects of Heat, with Obſervations on the Theories of Mixture*, in which all the general or leading points of this doctrine were expreſſed with abundant diſtinctneſs and preciſion; and the honour of the invention is given in very expreſs terms, to Dr. Black, Profeſſor of Chemiſtry at Edinburgh;—" a man," ſays the compiler, " whoſe modeſty is even ſuperior to his merit." Theſe ſpeculations might find their way to Stockholm in the ſame manner. I remember a very accompliſhed young gentleman from thence in 1768 or 1769, (I think his name was Willems or Willemſon) who was much in the company of Dr. Black and his friends. He was occupied ſolely with chemiſtry, and particularly with metallurgy. Beſides, manuſcript copies of Dr. Black's lectures were to be had for a moderate price, and were purchaſed by many ſtudents.

Should I be thought by ſome readers to have treſpaſſed unreaſonably on their time and patience, I ſincerely aſk their forgiveneſs. But I truſt that I ſhall have the thanks of all thoſe who have had the happineſs of attending the lectures of our amiable and excellent Profeſſor. I hope alſo that others will grant that I have only endeavoured to diſcharge my duty to his honoured memory. A more proper occaſion will preſent itſelf, in the conſideration of another article, for taking ſome notice of the ſilence of Mr. Lavoiſier, and Mr. de la Place on this head, when I truſt it will appear in what high eſtimation Dr. Black's ſpeculations on this ſubject were held by thoſe gentlemen, and how anxious they were to keep them from the public eye.

[*Note* 9. *p.* 217.]

This however is a very gratuitous opinion, and has always been admitted with great heſitation. Dr. Hooke exhibited at a meeting of the Royal Society in 1682, an experiment, in which it appeared that a mirror of foiled glaſs reflected the heat of a fire very feebly, although it reflected the light with great brilliancy. Mr. Scheele ſhewed that a plate of clear glaſs, held between the face and a glowing fire, interrupted for a while the whole heat, but tranſmitted the light without ſenſible diminution: And Dr. Herſchell has ſhewn, in the moſt convincing manner, the complete ſeparability of light and a determinate portion of the heat, by means of a difference in their refrangibility.

[*Note* 10. *p.* 219.]

Dr. Darwin directed a ſtream of air through a ſmall aperture of a tube intenſely heated, into a box, which had an optical apparatus in the oppoſite ſide, by which he could ſee any light at the point of the pipe with great advantage. Having ſtaid ſome time in the dark, to render his eye very ſenſible to light, he looked in, and cauſed the injection of heated air to be made; but he could not perceive the ſmalleſt light. He then placed a ſlender

bit of gold at the diſtance of half an inch from the hole. When the heated air was again injected, the gold acquired a very bright glow in a few ſeconds, although the heating air was altogether inviſible. Dr. Herſchell's curious obſervations ſhew us that, by a proper optical apparatus, a body may be intenſely heated by the rays of the ſun, without being at all illuminated; and it ſeems to require an experiment like Dr. Darwin's to decide whether *this* heat would cauſe it to emit light. Alſo, ſeeing that it is affirmed by the French chemiſts that oxygenous gas is the ſole ſource of the heat and light which appear in combuſtion, it would ſeem to be doubtful whether Dr. Darwin would have ſeen the gold ignited, had it been heated by a ſtream of azotic gas inſtead of common air.

[*Note* 11. *p.* 223.]

This poroſity of clay is ſuſceptible of conſiderable variety, by different modes of preparation. It is prepared for the better kinds of pottery, by beating it into a uniform, and almoſt fluid mud with water, and allowing this to ſettle at leiſure in a pot or ciſtern. By this treatment, all ſmall ſtones, and even coarſe ſand, fall to the bottom. When ſo much of the water has evaporated as to leave the clay fit for being formed in the mould, or on the wheel, and able to ſupport itſelf in ſhape, it may be dried in the air and ſun, without ſhrinking remarkably. When a veſſel of this conſiſtency is baked with a moderate red heat, ſuch as is given to bricks for inſide work, it is ſo porous that the water will run through it in a few minutes, but without taking away the ſmalleſt portion of it. This is very eaſily underſtood. It is owing to the great ſpace which had been occupied by the water. The ſubſequent contractions of ſuch baked clay by ſtronger heats are much greater than thoſe of clay which has been compreſſed by beating or kneading, after a greater portion of the water has evaporated from the prepared mud. I find, by ſome careful experiments with the fineſt clays, that a difference in this reſpect induces a very ſenſible difference in the total contraction of baked clay, even when very hard. I mention this circumſtance, becauſe, though ſufficiently obvious and intelligible, it does not ſeem to have been duly attended to by Mr. Wedgewood and others who employ this kind of thermometer; and it introduces conſiderable uncertainty in the compariſon of experiments made by different perſons. The different contractibility has a ſenſible relation to the ſpecific gravity of the baked clay, (varniſhed, to prevent imbibition in the trial), but I have not been able to aſcertain its degrees with any tolerable preciſion. I found very unexpected anomalies.

[*Note* 12. *p.* 236.]

Having occaſion, in autumn 1774, to go down and inſpect a drain in a coalwork, where an embankment had been made to keep off a lateral run of water, and, crawling along, I laid my hand on a very luxuriant plant, having a copious, deep-indented, white

foliage, quite unknown to me. I inquired of the colliers what it was? None of them could tell me. It being curious, I made a ſod be carried up to the day-light, to learn from the workmen what ſort of a plant it was. But nobody had ever ſeen any like it. A few days after, looking at the ſod, as it lay at the mouth of the pit, I obſerved that the plant had languiſhed and died, for want of water, as I imagined. But looking at it more attentively, I obſerved that a new vegetation was beginning, with little ſproutings from the ſame ſtem, and that this new growth was of a green colour. This inſtantly brought to my recollection the curious obſervations of Mr. Dufay; and I cauſed the ſod to be ſet in the ground and carefully watered. I was the more incited to this, becauſe I thought that my fingers had contracted a ſenſible aromatic ſmell by handling the plant at this time. After about a week, this root ſet out ſeveral ſtems and leaves of common tanſy. The workmen now recollected that the ſods had been taken from an old cottage garden hard by, where a great deal of tanſy was ſtill growing among the graſs. I now ſent down for more of the ſame ſtuff, and ſeveral ſods were brought up, having the ſame luxuriant white foliage. This, when bruiſed between the fingers, gave no aromatic ſmell whatever. All theſe plants withered and died down, though carefully watered, and, in each, there ſprouted from the ſame ſtocks freſh ſtems, and a copious foliage, and produced, among others, common tanſy, fully impregnated with the ordinary juices of that plant, and of a full green colour. I have repeated the ſame experiment with great care on lovage, *(leviſticum vulgare)* mint, and caraways. As theſe plants throve very well below, in the dark, but with a blanched foliage, which did not ſpread upwards, but lay flat on the ground; in all of them there was no reſemblance of ſhape to the ordinary foliage of the plant. All of them died down when brought into day-light; and the ſtocks then produced the proper plants in their uſual dreſs, and having all their diſtinguiſhing ſmells.

From ſuch experiments, I thought myſelf entitled to ſay that the ſun's rays not only produced the green fæcula of plants, but alſo the diſtinguiſhing juices, and particularly the eſſential oils. The improvements which have been made in chemical ſcience ſince that time, have, I think, fully confirmed my conjecture. The ſeparability of light and heat, firſt noticed by Dr. Hooke, and exhibited by him to the Royal Society, at the meeting of March 16. 1682; *(See Birche's Hiſt. of the Royal Society, vol. IV. p. 137.)* and, after being forgotten, again brought forward by Dr. Scheele, has been lately confirmed in the moſt complete manner by Dr. Herſchell's valuable experiments, in which he found the calorific rays leſs refrangible than the calorific. It was no unreaſonable ſuppoſition, therefore, that the ſun's rays contained phlogiſton, ſince we ſee that they contained more than light. Nor was my conjecture without ſupport from what was then known. The ſun's rays blacken the vitriolic acid, and from vitriolic acid much blackened we can obtain ſulphur. His rays render colourleſs nitrous acid ruddy and fuming, in the ſame manner as a drop of ſpirit of wine would have done. They deſtroy vegetable colours. They blacken luna cornea, and other metalline ſalts or oxyds, in the ſame manner nearly as fatty ſteams and vapours do. Theſe are ſtrong indications of ſome material emana-

tions from the ſun, and are in no manner explicable by any mechaniſm of elaſtic undulations. Theſe phenomena cannot be conceived by the mind as the effects of an undulation. They reſemble communications of matter. But, indeed, the proportionality of the ſines of incidence and refraction has always appeared to me to give the ſtrongeſt proof that the ſun's rays are emiſſions of moveable, inert matter, affected, as all other matter is affected, by what we call moving forces.

The new doctrines in chemiſtry make no change in the inferences from theſe phenomena. It is not admitted that phlogiſton is communicated to nitric acid by the ſun's rays when they render it fuming; but they are ſaid to detach oxygen. The whole train of chemical phenomena induce us to conclude that oxygen is diſengaged, either by being detached by ſomething now combined with the remainder, or that the thing furniſhed by the rays makes part of the gas now formed.

When I firſt heard of Scheele's curious experiment of throwing the priſmatic ſpectrum on the nitrate or muriate of ſilver, in order to compare the blackening powers of the differently colouring rays, it revived my chemical ſpeculations, from which I had long deſiſted; and, in beginning to repeat his experiment, an accidental circumſtance ſuggeſted to my mind an experiment which I thought deciſive of the queſtion. "Is light an ema-"nation of matter from the ſun, or is it the undulation of an elaſtic medium?" I propoſed to form an image of the ſun on the nitrate of ſilver, by means of rays collected by a metalline ſpeculum, and made to paſs through a glaſs veſſel filled with clear and colourleſs nitrous acid. On the ſuppoſition that the preparation of ſilver is blackened, and nitrous acid rendered fuming, by the ſame principle in the ſun's rays, I concluded, that when they had performed one of thoſe taſks, they could not be in a condition to perform the other, at leaſt in the ſame degree. I accordingly began the experiment, and found a remarkable diminution of the effect by the interpoſition of the acid. But I could not make any very confident inference from this, having long before that time found that the ſeparation of heat and light by a tranſparent body, ſuch as Scheele's pane of glaſs, was chiefly owing to abſorption of the heat by the glaſs, which, after it was ſaturated, tranſmits the heat copiouſly, along with the light. It therefore required a train of experiments to determine how much was owing to this cauſe. Before I could accompliſh this object, the failure of my health, in the ſpring of 1787, put a full ſtop to all active inveſtigations and purſuits.

My thoughts on this ſubject have again been rouſed by the obſervations of Meſſrs. Bockmann and Ritter, in Germany, and Mr. Wollaſton in England. They have obſerved that the muriat of ſilver was ſtrongly darkened by rays of the ſun which are more refracted than even the violet rays. The muriat was affected in a ſpace lying beyond the violet light, in the ſame manner as Dr. Herſchell had obſerved the thermometer affected in a ſpace equally in the dark, beyond the red rays of the ſpectrum.

Thus it appears that there are rays which illuminate, rays which warm without illuminating, and rays which affect bodies chemically without producing either light or

heat. This obſervation bids fair for giving us more knowledge of the nature of combuſtion. And, if this newly diſcovered principle ſhall be found neceſſary for this moſt remarkable phenomenon of nature, it may very well be called *the phlogiſton*, in whatever way it contributes to the effect. And theſe facts ſeem to recommend the proſecution of my experiment. It would be proper to compare the darkening power of the rays coming through nitric acid with that of rays coming through as much water. The rays produce a remarkable effect on the firſt, and none on the laſt.

But, whatever may be the reſult of ſuch experiments, it remains certain that the rays of the ſun, and the light of day, affect plants in the manner deſcribed above, and in ſome way or other are neceſſary to the production of their moſt combuſtible ingredients. The plants may be ſaid to feed on the light by their green leaves, as much as they do on the juices of the ſoil by their roots. As the roots are protruded all around through the yielding ſoil, in queſt of nouriſhment from the earth, ſo do the plants direct their growth, and turn the upper diſk of their green leaves to the light, and to the light alone.

[*Note* 13. *p.* 240.]

This theory, ſo oppoſite, as Dr. Black obſerves, to the theory of Stahl, is not ſo recent as is generally imagined. It was ſeen, in all its extent and importance, by Dr. Robert Hooke, one of the greateſt geniuſes and moſt ardent inquirers into the operations of nature, who figured during the latter half of the ſeventeenth century, a period full of great diſcoveries.

Dr. Hooke propoſed this theory in conſiderable detail in his *Micrographia*, publiſhed in 1665; *(ſee p.* 103.*)*; and in his *Lampas*, publiſhed in 1676; and he makes it an important doctrine in his treatiſe on Comets, and in many paſſages of his Cutlerian Lectures. He promiſes to take it into ſerious conſideration, and to publiſh a full exhibition of it. The alluſions made to it in his Lectures make it evident that he had continued to make ſome deſultory additions to his firſt conceptions. His *Lampas* contains a moſt accurate explanation of flame, which cannot be ſurpaſſed by any performance of the preſent day.

In the Micrographia he ſtates the theory in the following words:

1. The air in which we live, and breathe, and move, and which encompaſſes and cheriſhes all bodies, is the univerſal ſolvent of all ſulphurous (ſynonymous, at that time, with inflammable) bodies.

2. This action it performs not till the body be ſufficiently heated, as we obſerve in other ſolutions.

3. *This action of diſſolution produces the great heat which we call fire.*

4. It acts with ſuch violence as to agitate the particles of the diaphanous body air,

and to produce that elaſtic pulſe called light. *(See his own hypotheſis concerning the propagation of light).*

5. This action, or diſſolution of inflammable bodies, is performed by *a ſubſtance inherent in, and mixed with the air, that is like, if not the very ſame, with that which is fixed in ſaltpetre.*

6. In this diſſolution of bodies by the air, a part of the body, uniting with the air, is diſſolved or turned into air, and eſcapes and flies about.

7. As one part is thus turned into air, ſo another is mixed with it, but forms a coagulum, or precipitation, ſome of which is ſo light as to be carried away with the air, while other groſſer and heavier matters remain behind, &c. &c. This latter article is frequently employed in other parts of his writings, and is ſometimes called a groſſer *compound*, mixed with matters terrene, and originally inſoluble in air, and incombuſtible.

Can any thing more be wanting to prove that this is the ſame with the modern theory of combuſtion? Nothing but to ſhew that this coagulum contained the air which had formed it, by ſhewing an increaſe of its weight, or by ſeparating it again. But the eager mind of Hooke, attracted by every appearance of novelty, was ſatisfied with the general notion of a great ſubject, and immediately quitted it in chace of ſome other intereſting object. Had he not been thus led off by a new purſuit, this wonderful man would not only have anticipated, but completed many of the great diſcoveries of the laſt century. It was a bold conception, and only a vigorous mind could entertain it for a moment, that the vaſt heat of combuſtion was contained in a few grains of air. Yet this was his opinion, as appears by the explanation which he gives, in various meetings of the Royal Society, and in his lectures on comets, of the deflagration of combuſtible bodies with ſaltpetre, and of fiery motion.

In the treatiſe called Lampas, he obſerves that this his treatiſe, publiſhed eleven years before, had been very favourably received, and that he had not ſeen any valid objection offered to it. It was in this interval that Dr. Mayhow, at Oxford, publiſhed his book *De Sale Nitro, et Spiritu Nitro-aëreo*, in which he holds preciſely the ſame doctrine; but his exhibition of it is obſcure, complicated, and wavering, mixed with much mechanical nonſenſe, of wedges, and darts, and motions, &c. according to the faſhion of the times. Hooke's conception of the ſubject, on the contrary, is clear, ſimple, and ſteady. The only addition made by Mayhow are ſome obſervations on the increaſe of weight obſerved in the preparation of diaphoretic antimony, &c. Hooke, explaining at a meeting of the Royal Society, ſome tricks of the plumber's workmen, who called the litharge which formed on the ſurface of melted lead droſs, and took it with them as their perquiſite, ſays expreſsly, that they can make droſs of the whole, and that it is more than the lead by all the air which was its menſtruum. But Mayhow wrote on the ſubject expreſsly, and it appears in the title of his book. He is remembered, while Hooke is forgotten, becauſe no one would think of looking into the Micrographia for chemical infor-

mation. The theory comes in by chance, to explain the indeſtructibility of charcoal in cloſe veſſels by heat. Mayhow alſo made many very ingenious experiments on the air which had contributed to inflammation, and has anticipated both the manipulations and the diſcoveries of modern pneumatic chemiſtry.

I do not know a more unaccountable thing in the hiſtory of ſcience, than the total oblivion of this theory of Dr. Hooke, ſo clearly expreſſed, and ſo likely to catch attention. No notice that I know of had been taken of it, till I mentioned it in the ſupplement to the Cyclopædia Britannica, in the ſketch given of Dr. Herſchel's diſcovery of the radiation of ſolar heat, in the article *Thermometrical Spectrum*. I made the obſervation in 1798, having then got a copy of Hooke's Lampas, and other tracts, which are as curious as they are rare. And I then requeſted Sir Joſeph Banks to order a ſearch among Dr. Hooke's papers, in the poſſeſſion of the Royal Society. I am perſuaded that many of his ſpeculations on this ſubject will be found among them, and I earneſtly requeſt an inſpection of them to be made.

I truſt that the reader is not diſpleaſed with this my endeavour to do juſtice to the well founded claim of our illuſtrious countryman. Had Dr. Hooke's fortune allowed him to multiply experiments, to aſſociate in his labours a knot of eminent aſſiſtants, and to form *a party* in the Society, ready at all times to meet and to document every occaſional diſcovery, I make no doubt but that he would have ſtood next to Newton in philoſophical rank, but "his lot forbade." Other *eminent* men would willingly have benefited by his genius, but would not be his ſeconds. They uſed him extremely ill; and this increaſed his diſtruſt and jealouſy. He does not ſeem to have had the leaſt ambition to head a ſect, although he ſometimes boaſts too much of his own inventions. Not incited by this ambition, all his efforts were ſolitary,—merely to quench his own ardent thirſt for knowledge. His temper, ſoured by ill uſage, and by the infirmities of a moſt feeble and ſickly conſtitution, made him quarrel with all his friends in the Society; and, in ſo doing, he was guilty of ſome ingratitude; for he had been encouraged, protected, and even borne with, with ſome tenderneſs, on account of his genius and his infirmities. But all theſe things contributed to the neglect of his writings, and this took away the chief incitement to the proſecution of this ingenious theory. He did not, however, altogether give it up; for ſome of his occaſional employments of it allude to recent obſervations. I am perſuaded that valuable thoughts on this ſubject are ſtill to be found in the cheſt of papers committed to the keeping of the Society by Mr. Waller, after he had extracted from them what he has publiſhed as Dr. Hooke's poſthumous works. Dr. Hooke died in peace, and his memory was honoured by his colleagues in the Society. Mr. Lavoiſier fell a ſacrifice to the ambition of thoſe whom he had aſſociated with him in his reſearches, all of whom either were at the time of his death, or became, in a few months, miniſters, or generals, or perſons in high public functions. France, indebted to this man for the only honour that it has acquired dur-

ing the eventful period of the revolution, condemned him to death *for putting water into the tobacco, and other things hurtful to the health of the citizens.*

——Quis talia fando
Temperet a lachrymis?——

[*Note* 14. *p.* 242.]

When the diſh is filled to the brim, ſo that the air may have free acceſs to the burning fuel, and we take care, at the ſame time, to hinder it from being thrown into eddies, by ſetting a glaſs cylinder round it, at a ſmall diſtance, allowing the air to get in between it and the diſh, we ſhall obſerve the flame occupy the whole circumference of the diſh. But if a ſmall bit of paper be lying on the middle of the ſpirits, or even ſupported at a ſmall height above it, we ſee that it is not burnt, though ſurrounded by flame. It is only at the outer ſurface of the aſcending vapour that the inflammation is going on. What is within cannot burn, becauſe there is no air in contact with it. If we hold a glaſs plate above it, ſo as to hinder the flame from riſing high, and we then look down through the glaſs, we ſhall ſee that none of the internal part is burning. But if we introduce a pipe through this film of flame to the middle, we ſhall ſee a ſtream of flame ariſe from the end of this pipe, and we are ready to believe that there is a ſtream of inflammable matter coming from the pipe. But it is only air which comes out, and kindles what it touches. This flaming ſtream from the pipe is alſo only a cylindrical film of flame like the other. Theſe obſervations and remarks enable us to explain the upright and tapering form of all flames. Flame riſes, becauſe it is a hot vapour, which expands the contiguous air, which therefore riſes by its ſpecific levity, and carries up with it this ſhining vapour. Its form is tapering, becauſe it is luminous only where it is really burning, that is, in contact with the air. The exterior ſtratum is burnt before it gets far up. The next riſes till it overtops the outer one, now burnt, and it burns in its turn, getting a little higher before it is conſumed. The third ſtratum muſt get above the ſecond, before it begin to burn, and it adds a little more to the height of the flame: And thus the ſtrata burn at top, in ſucceſſion, and the centre one cloſes the pyramid. Dr. Hooke gives a moſt accurate deſcription and explanation of all the phenomena of flame, illuſtrated by a figure, in the *Lampas*, p. 5. proving it to be a ſhining film of inflamed vapour.

In conſequence of this ſuperficial conſtitution of a flame, whenever we cover it with any thing which prevents the acceſs of freſh air, it is inſtantly extinguiſhed. But an inflammable body that is ſolid, and fixed in a great heat, may be ſuperficially extinguiſhed in this way. But if the cover be taken off immediately, the ſurface, which appears black at firſt, ſoon grows red again, by the heat of the interior parts, and the combuſtion recommences.

Combuſtible bodies which have an intermediate degree of volatility burn with a flame

more or leſs violent. But when a conſiderable portion is flaming at once, it rarely happens that the combuſtion is complete in the interior parts of the flame. Much of this vapour is only ſcorched, or charred, and is hurried away in this condition by the current of air generated by the heat. This charred vapour is what we call *ſmoke*, and *ſoot*, both of which are ſtill highly inflammable. The ſure way of producing a more complete inflammation is to prevent a great quantity of ſurface from flaming at once. Thus, a very ſmall wick is found to burn without ſmoke or ſoot; and Argand's lamp, which admits a current of air into the centre of the flame, produces the ſame effect in an eminent degree. I employed ſuch a lamp in 1767–8. It conſiſted of two concentric circles of wicks of ruſh pith ſtuck on pins. The air came up in the centre, and between the circles. This was the lamp of a diſtilling apparatus, and ſtood on the laboratory table of Glaſgow college for two years. Quitting all chemical purſuits in 1769, I thought no more of it.

[*Note* 15. *p.* 265.]

For Sir Iſaac Newton, in ſtating this violent conflict of forces, in order to account for the turbulent motions really obſerved, did not proceed as many of his followers have done, and *ſuppoſe* the force to be great, becauſe he had a violent motion to account for. He firſt prepares the way, by proving that, in the corpuſcular actions of bodies, we *obſerve* forces which are inconceivably great. He ſhews that the ſum total of the force with which one grain of glaſs attracts the light which it refracts, is not leſs than the force with which a wire would be ſtretched by a hundred thouſand millions of millions of pounds of matter hanging by it. This is ſurely competent to the production of the moſt violent exploſion that chemiſtry preſents to our wondering view. Obſerve too, that it did not eſcape the remark of Newton, that *this identical force* is concerned in all the moſt violent effects of chemical mixture. They are accompanied by the extrications of heat and of light. When we come to conſider ſome of theſe motions in detail, we ſhall be better able to appreciate the wonderful ſagacity and penetration of this extraordinary man. We ſhall ſee with diſtinctneſs why argentum and mercurius fulminans, while they explode with a force certainly greater than that of gunpowder, cannot propel a bullet with half the force which it can impart.

[*Note* 16. *p.* 267.]

It will prevent many miſtakes if we adhere ſtrictly to this meaning of the term; if we conſider it merely as an abbreviated way of ſaying that the particles of bodies do really thus come together. We ſhall find abundant employment in aſcertaining with preciſion all the caſes in which this action is to be obſerved, the limits to its generality, and the peculiar modifications of it, which diſtinguiſh certain claſſes of this phenomenon, and the caſes or ſubſtances which exhibit theſe diſtinctive modifications;—and this employment

will advance our chemical knowledge incomparably more than any ſpeculation about its cauſe. At the ſame time, I ſee ſome advantages attending the uſe of the term *chemical affinity*. It diſtinguiſhes very compendiouſly the phenomena of combination (which are the chief objects of chemiſtry) from the phenomena of coheſion, adheſion, capillary attraction, &c. which are affected by forces perfectly ſimilar, but are not ſo characteriſtically chemical. But further,—attraction ſurely is the drawing one thing toward another. Now there are many inſtances where the bulk of the compound exceeds the ſum of the bulks of the ingredients. The particles, therefore, have not been drawn towards each other. Affinity does not, in general, imply any ſimilarity. When it is ſo uſed, we are ſenſible of ſome degree of figurativeneſs or metaphor.

But whether we employ the word attraction or affinity, we muſt be careful to attach no other idea to it but that of a fact, a determination of each particle of a certain ſubſtance to unite chemically with each particle of another certain ſubſtance, ſo as to conſtitute particles of a compound of both. We run great riſk of miſtake, if we adopt any notion of the mode of action by which this union is effected. The late diſcoveries of the chemical phenomena called *galvanic*, ſeem to derange all our notions of the immediate agent in chemical union or ſeparation.

[*Note* 17. *p.* 267.]

I think that it will be of ſervice to remark once for all, that we have no denominations for theſe forces which we conceive as the immediate agents in natural phenomena, except the phenomena themſelves, which we conceive them to produce. We call them the force of *gravity*, *coheſion*, *refraction*, *&c.* In a few caſes, we name them by the ſubſtances in which they ſeem to reſide, as *electricity*, *magnetiſm*. This circumſtance alone ſhews that we have no direct knowledge of the nature and mode of action of thoſe forces; they are not objects of our obſervation; and our knowledge of their ſuppoſed effects, that is, our knowledge of the phenomena, is all that we ſhall ever acquire of the forces or ſuppoſed cauſes. When we go, or think that we go, a little farther, and call the force of coheſion an *attraction*, we mean nothing more than that the effect reſembles the effect of a real attraction, or pulling a thing toward us with our hand.

[*Note* 18. *p.* 269.]

In employing the words *ſtronger* and *weaker*, as expreſſing qualities of the chemical attractions, we muſt be careful not to uſe them preciſely in the ſame way as in mechanics. It muſt be uſed merely as an expreſſion of *prevalence*, but not as expreſſing a meaſurable quantity, to which we give the name of *intenſity*, otherwiſe we ſhall run the riſk of miſtake in our conceptions of thoſe quantities. Aquafortis ſeparates the parts of iron, and we expreſs this alſo, by ſaying that the attraction of iron for aquafortis is ſtronger than its coheſive attraction for its own parts. We ſay the ſame thing of potaſh,—and we ſay

that the attraction of aquafortis for potaſh is ſtronger than its attraction for iron, becauſe potaſh ſeparates aquafortis from iron. Theſe expreſſions might oblige us to ſay that the coheſive force of iron is leſs than that of potaſh, did we not know the contrary. The *prevalence* is all that we ſhould conceive to be expreſſed, and this rather ariſes from the peculiar mode of action, than from the greater intenſity. Perhaps there is nothing in many of thoſe chemical changes analogous to what we call reſiſtance and counteraction in mechanics. We ſee, indeed, ſome caſes in which we cannot doubt it, as in the prevention of vaporiſation (and perhaps of congelation in ſome caſes) by external preſſure.

[*Note* 19. *p.* 273.]

Dr. Black ſometimes explained the effect of ſtagnation, and the propriety of agitating the mixture, at length, but has not committed his view of the matter to writing. I recollect diſtinctly the explanation which he gave in 1765 at Glaſgow. A tall beer glaſs being filled with water, a long funnel was put into it, whoſe pipe reached to the bottom, and as much of a ſaturated and deep-coloured ſolution of blue vitriol was poured in, as occupied about an inch at the bottom of the glaſs. It was ſet on a bracket, having a white paper behind it. This was done ſome days before this lecture, and the ſtudents were made to obſerve how the colour ſlowly aſcended upwards, growing fainter and fainter as it was farther up the glaſs. In this lecture Dr. Black obſerved that this gradation of colour ſhewed that the different horizontal ſtrata of the fluid were in different ſtates of ſaturation, and becauſe the colour did not change by ſenſible ſteps, but gradually, the difference between two contiguous ſtrata in reſpect of ſaturation was infinitely ſmall. Therefore, ſince the attractive force of a ſtratum was ſo much the greater, as it contained leſs of the ſalt, and as the adjoining ſtratum contained more, it muſt follow that, in any horizontal ſection, the force drawing the ſalt upward muſt be extremely ſmall, and the motion extremely ſlow; and that the only method to make it proceed faſter, is to bring the parts which contain leaſt ſalt into contact with thoſe which contain moſt. As a proof of his doctrine, he kept the glaſs clear of all diſturbances, on the bracket; and three months after, the colour had not riſen three inches. Indeed it did not reach near the top after three years. Dr. Black explained the ſlow communication of heat along the parts of ſolid bodies, and the effects of ſtagnation in the atmoſphere, in the ſame manner.

[*Note* 20. *p.* 275.]

It may be ſaid here that this manner of generaliſing the events is gratuitous, and that we have no other teſt of the proportional ſtrength of the chemical forces concerned in theſe phenomena but the phenomena themſelves. We call that a ſtronger force which ſeparates two ingredients united by the force which we term the weaker. It muſt be acknowledged that this generally has been the evidence on which the proportion of the attractions has been eſtabliſhed; and it is legitimate, and is equally true in mechanics and

every department of phyſics. But it is not our ſole authority, even in the chemical actions of bodies. It is perhaps true without exception, that, when theſe different combinations (which may be thus changed in ſucceſſion by a third body) can alſo be deſtroyed by the varied force of one agent, ſuch as heat, we ſhall find that thoſe ingredients which manifeſt the ſtrongeſt *elective* attractions really require the greateſt exertions of this general agent to ſeparate them. The exceptions ſeem, at firſt, to be numerous and remarkable; but when the co-operation of different degrees of fluidity and volatility are conſidered, almoſt the whole of the exceptions are found to come under the general rule.

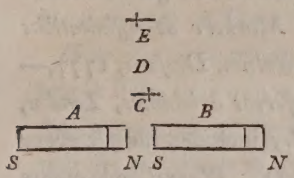
[*Note* 21. *p.* 276.]

When Sir Iſaac Newton took that curſory view of chemical compoſition and decompoſition which he has expreſſed in the queries annexed to his Optics, he ſaw well how vain an attempt it would prove to reaſon minutely on thoſe forces on mechanical principles. For he had already found that his own utmoſt efforts could only approximate to the ſolution of the problem of three particles acting on one another. Nor have the united efforts of all the mathematicians of Europe, ſince his time, produced an accurate ſolution of this celebrated problem. What then can we hope for, when we muſt conſider at once the combined action of myriads of particles? He contented himſelf, therefore, with conſidering the phenomenon of chemical combination as the only acceſſible mark of the acting force, and recommended the ſtudy and compariſon of theſe as the only means of improving the ſcience. It was, as yet, far removed from firſt principles; and the ſimpleſt phenomenon preſented to our view is a very complex event. The only proper method of proceeding is to compare them, and claſs them according to their extent and generality, in the hopes of finding points of reſemblance, which would form a law ſtill more general, and therefore nearer to the firſt principles of the ſcience. The ſeparation by a third body is a very general law, and it did not appear neceſſary, or even proper, to ſpeculate on its cauſe.

Notwithſtanding the eager endeavours of his followers to account for the ſeparation, or for the extreme rarity of a combination of three ingredients in a ſeries of elective attractions, no ſatisfactory explanation has yet been obtained. Few chemiſts were mechanicians, and thoſe who were eminent mathematical philoſophers, ſuch as Keil, Bernouilli, and a few others, were but ſorry chemiſts. Boſcovich is the firſt, who, by conſidering corpuſcular force in the moſt general and abſtract manner, has ſhewn how the action of two particles may be totally annihilated by the action of a third, and this, even although the action of this third on either of the other two, be in any degree *weaker* than the mutual action of thoſe two. He ſhews that this will depend as much on the kind, as on the general magnitude or intenſity of the force.

It may perhaps aſſiſt the imagination a little to point out ſome examples of the ſame kind which we can explain to a certain degree. When a piece of iron is hanging by the

north pole of a magnet, if we preſent the ſouth pole of another magnet to the place of junction, the iron immediately drops. Moreover, if we hang to a magnetic pole a piece of iron as large as it can carry, and then attempt to hang another ſuch piece beſide it, the firſt will certainly drop from it. Again, we know that a metal plate, negatively electrified, attracts a rod of metal very ſtrongly. We know that a plate poſitively electrified does the ſame. Yet we alſo know, that a charged plate of glaſs, one ſide of which is *moſt highly* poſitive, and the other as highly negative, exerts ſcarcely any attraction on the metal rod. And we underſtand in a conſiderable degree how theſe things happen. A ſtill more appoſite illuſtration may be had by the following curious phenomenon in magnets, which had not been noticed till I mentioned it in the article *Magnetiſm*, of the Cyclop. Britan. § 29.



Having laid two good magnets, A, B, of equal power, on the table, with the north pole of A near the ſouth pole of B, and the two magnets forming a ſtraight line, take a very ſmall compaſs needle, ſuch as are frequently mounted for wearing on a watch chain, and place it at C. The needle will immediately place itſelf parallel to the magnets A and B, and its poles will be in the ſame direction with theirs. Draw it out perpendicularly from the two magnets, and you will find its polarity gradually diminiſh, as you may perceive by its vibrations growing ſlower. When at a certain diſtance, at D, its virtue ſeems entirely gone, for it will remain in any poſition that is given it; draw it ſtill farther off, and you will perceive its virtue returning again, but its polarity is reverſed. At E, as far from D as D is from C, it will ſtand in the poſition contrary to that of C.

While the needle is at E, gently draw the magnets farther aſunder, both equally from the middle point between them, and keeping them ſtill in the ſame line. When at a certain diſtance, the virtue of E is again deſtroyed, and it will retain any poſition. And, on ſeparating them ſtill farther, E recovers the ſame polarity that it had in C.

From this experiment, we ſee how much the mutual relations of particles endowed with powers like polarity, depend on the preciſe poſition, and the peculiar conſtitution of the ſubject which affects them. If, inſtead of one little magnetic needle which we place ſucceſſively at C, D, and E, we employ ſeveral, turning on pins ſtuck into little pedeſtals, we ſhall obſerve them all affected, and attracting each other by their neareſt poles; and when the diſtance of A and B is properly changed, the virtue of the little needles is alſo changed, every pole repelling what it formerly attracted.

[*Note* 22. *p.* 279.]

In all theſe caſes the progreſs of chemical ſcience has been greatly obſtructed by the attempts to reaſon from higher principles. We know, and ever ſhall know, ſo very little about the mechaniſm of chemical attraction, that we can only frame hypotheſes, and then reaſon from ſuch hypotheſes. No acceſſion of knowledge is obtained by this procedure.

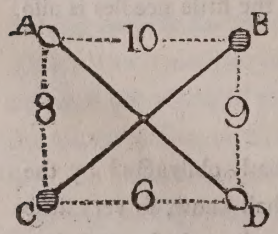
For our hypotheſis, in order to have any influence on our mind, muſt be founded on a certain extent of reſemblance to the phenomena; and its whole value depends on that reſemblance. We have the phenomena themſelves; and *their* coincidence is our *only* general law. The hypotheſis is only a fanciful picture of this law: It is therefore uſeleſs. It does not ſupport the phenomena, but is founded on them, if it be good for any thing. In the preſent caſe Mr. Berthollet has wiſely avoided reaſoning from any ſuppoſed properties of chemical attraction, and deduces all his general laws from extenſive coincidences of facts.

[*Note* 23. *p.* 279.]

Beſides the authors mentioned by Dr. Black, many uſeful remarks and general views may be gathered from the tables of other eminent chemiſts. *Marherr de Affinitatibus Corporum, Viennæ*, 1762,—and particularly *Wenzel Doctrina Affinitatum, Dreſden*, 1777,—and *Kirwan on the ſpecific Gravities and attractive Powers of different Subſtances, London*, 1781,—deſerve a careful peruſal. The table in *Gren's Chemiſtry* includes almoſt every article, and every particular view that has been taken of the ſubject. *Wenzel* endeavours to prove that all the diſtinguiſhing attractions of different ſubſtances are only modifications of the general attraction of gravitation; and are modifications ariſing from the figure and ſtructure of the ultimate particles of each ſubſtance. But Wenzel had not the mathematical knowledge neceſſary for ſuch a taſk. Other authors have had the ſame notion. And no doubt, the diſtinguiſhing actions of different ſubſtances may have this origin,—that is, may depend on the figure and ſtructure of the particles, although the primary atoms, of which theſe conſiſt, have but one law of action. This is the great principle of Abbé Boſcovich's theory. But I think it ſtrictly demonſtrable that no figure or ſtructure will conſtitute ſuch particles as we obſerve, if the action of the primary atoms be an attraction in the ſimple ratio of the ſquare of the diſtance inverſely; and I am certain that no repelling particles, or repelling body, can ever ariſe in this way.

[*Note* 24. *p.* 280.]

This moſt intereſting law of chemical combination may be conceived, if not explained, in the following manner:



When a compound conſiſting of the ingredients A and B is mixed with another, of which the ingredients are C and D, there are two affinities or forces ſuppoſed to be in action; one connecting A with B, and the other connecting C with D.

But let us alſo ſuppoſe that A has an affinity to C, or at-

tracts it as well as B, and that B alſo attracts D. Then, in the mixture we have two other forces, one connecting A with C, and the other connecting B with D. We may expreſs theſe forces by the ſymbols A×B, C×D, A×C, B×D.

The two firſt may be called *maintaining forces*, becauſe they tend to keep things in their preſent condition. The forces A×C and B×D may be called *divellent*, becauſe they are conceived to have a tendency to ſeparate A from B, and C from D. This ſuppoſition is founded on the authority of the general fact of ſeparation of two ingredients by preſenting a third ſubſtance which has affinity to one of them.

From this manner of conceiving the ſubject, it is inferred that if the ſum of the forces A×C and B×D, exceed that of the forces A×B and C×D, we ſhall have a double exchange, and two new compounds. This may happen, therefore, although the attraction of A for B ſhould exceed its attraction for C, provided it do not exceed it ſo much as the attraction of D for B exceeds its attraction for C.

Since it is a general fact in chemiſtry, that ſubſtances act moſt powerfully in their ſimple ſtate, it may appear ſtrange that C, when already united with D, ſhould overcome the ſtronger attraction of A for B, which it cannot do alone. This ſhews us, by the way, that the *prevalence* in chemical action depends rather on the manner and concomitant circumſtances of the action, than on the meaſurable intenſity or magnitude of a particular force. Dr. Cullen uſed to repreſent this *chemical fact* by the diagram in the margin, where the numbers placed between the ſubſtances expreſs the ſuppoſed attractive forces exerted between the ſubſtances. This diagram ſuggeſts the notion of bodies attached to the ends of two rods or levers A D and B C, moveable round their interſection E. Were this the caſe, it is certain that the attraction of A for C, and of B for D, tend to ſeparate A from B, and C from D; and what is aſſerted above will happen,—the levers will cloſe between A C and B D, and A will apply itſelf to C, and B to D. Dr. Black firſt employed this diagram,—but he gave it up, becauſe it ſuggeſted a notion not chemical, but mechanical. Levers can have no place here. It ſuggeſts alſo an erroneous notion. The levers produce the effect, only in conſequence of a connection which they eſtabliſh between A and D, and between B and C. Now, in by far the greateſt number of caſes in which a double exchange is obſerved, we know of no ſuch connection. He uſed to expreſs the caſes more in the ſtile of chemical phenomena, by ſaying that, in order to have a double exchange, the *partiality* of D for B muſt exceed that of A for B. But ſtill this is merely a figurative expreſſion of an unknown cauſe.

It is not eaſy to conceive any mode of operation which will clearly produce the obſerved effect. It confirms, I think, the conſiderations mentioned in note 21. p. 276. and it receives ſome illuſtration from the magnetical phenomena mentioned there.

When we ſay that a double exchange happens when A×C+B×D exceeds A×B+C×D, we ſeem to ſay ſomething like inſtruction. But, in truth, it is only an inference of this greater partiality from the obſerved effect. We have generally no other

argument for ſaying that the exceſs of D's attraction for B above its attraction for C, is greater than the exceſs of A's attraction for B above its attraction for C. The fact is that we have inferred all theſe partialities only from the obſerved double exchanges.

Still, however, theſe double affinities are among the moſt intereſting chemical laws, and of immenſe uſe for operating changes which are otherwiſe impoſſible. Therefore a copious collection of the caſes in which they are obſerved will be a vaſt acceſſion to chemical ſcience, eſpecially if judiciouſly arranged.

Did we know every caſe in which one body can detach another, ſo as to make up a table of ſingle affinities complete, we might then, by comparing them in parcels of four, tell beforehand whether there will be a double exchange: But our knowledge is not yet ſo far advanced.

No where are the double exchanges ſo frequent as in the ſpontaneous fermentations of animal and vegetable juices. A fertile imagination, by employing four agents in this way, and aſſigning a convenient order of their action, may bring out almoſt any ultimate reſult he pleaſes. It is, therefore, a delicate part of the ſcience. There are ſome very judicious obſervations on the marks and characters of double affinities in Dr. Lubbock's inaugural diſſertation, *De Principio Sorbili*, which a philoſophical chemiſt ſhould be very clearly acquainted with, otherwiſe he is liable to many great miſtakes.

We muſt multiply as much as poſſible the caſes of double exchange by experiment; and we muſt apply to each caſe numbers which will agree with the obſerved prevalence. We muſt correct and recorrect thoſe numbers, ſo that the ſame number may always belong to the ſame ſubſtance, and yet ſuit its place in every caſe where that ſubſtance is one ingredient in a double exchange. This is a long and difficult taſk; but till it be accompliſhed, there is no certainty in the ingenious explanations given of many complex phenomena; eſpecially where a change of temperature makes a change in the chemical attractions.

[*Note* 25. *p.* 304.]

It may be carried much farther than this, by carefully preventing diſſipation. We ſee what conſiderable heats are produced in the common garden glaſſes, and in green-houſes, merely by preventing the air from abſorbing it and carrying it off. Mr. Sauſſure (*Voyage ſur les Alpes*, II. 932.) made a little box, lined with fine dry cork, whoſe ſurface was nicely and uniformly charred, to make it black, and alſo more ſpongy and unfit for conducting heat. It was covered by a very fine and thin plate of glaſs. A thermometer being laid on the bottom, and the box ſet in the ſun's rays, when the temperature of the ſurrounding air was 75° Fahrenheit, the incloſed thermometer roſe to 201° in a few minutes. The account is ſomewhat equivocal, in the way of expreſſing the change on the thermometer; and I ſee ſome foreign writers conclude that it roſe to 229°, 17° above boiling water.

I conſtructed an apparatus of this kind, employing three very thin veſſels of flint-glaſs,

which tranſmits much more heat (according to Herſchel's experiments) than any other ſpecies of glaſs. They were nearly of ſimilar ſhapes, arched above, and there was about one-third of an inch of an interval between them. They were ſet on a cork baſe, prepared like Mr. Sauſſure's, and ſet on down contained in a paſteboard cylinder. With this apparatus I have often, in a clear ſummer day, raiſed the thermometer to 230°, and once to 237° of Fahrenheit's ſcale. Even when ſet before a bright fire, it raiſed the thermometer to a boiling heat. When I took the apparatus into a damp cellar, before I ſet the glaſſes in their place, ſo that the air between them was damp, I found that when expoſed to the ſame degree of the ſun's light, I never could raiſe the thermometer above 208°; whence I concluded that damp air conducted heat much better than pure dry air. I did not find that inflammable air, or hydrogenous gas was ſenſibly inferior in its conducting power, notwithſtanding its great rarity. When I made theſe experiments, I did not know the remarkable effect of obſtruction to the motion of the air, noticed by Count Rumford.

[*Note* 26. *p.* 346.]

There are among Dr. Black's papers ſeveral ſketches of an arrangement of the chemical ſubſtances, with a view to make the conſideration of them more ſynthetical and ſyſtematic. One of them, in which he has made many changes and corrections, begins with water, and goes on in this manner:

" Water—Margraaf's experiments—ſilex, foreign to it—Dr. Auſtin, with iron filings " —inflammable air—ignifying air—Cavendiſh—water—nitrous acid—nitrous air, or " choke-damp—acids."

I think that I perceive the connections by which this may be made a ſkeleton of chemical philoſophy. There is another paper, in which the properties of nitric acid are gradually developed and applied, ſo as to terminate in the eſtabliſhment of the antiphlogiſtic ſyſtem, in a very familiar and eaſy manner.

I recollect a converſation which I had with Dr. Black, and Dr. James Hutton, ſoon after the appearance of Mr. Lavoiſier's ſketch of a ſcientific arrangement of chemical objects. I was telling him how highly I was pleaſed with that arrangement, on account of that happy train of ſynthetic deduction which it enabled Mr. Lavoiſier to carry through the whole of the chemical hiſtory of bodies.—

" This," ſaid Dr. Black, " is the very thing I diſlike it for. Chemiſtry is not yet a " ſcience. We are very far from the knowledge of firſt principles. We ſhould avoid " every thing that has the pretenſions of a full ſyſtem. The whole of chemical ſcience " ſhould, as yet, be analytical, like Newton's Optics; and we ſhould obtain the connecting " principle, in the form of a general law, at the very end of our induction, as the reward " of our labour. You blamed, and, in my opinion, juſtly, De La Grange's *Mechanique* " *Analytique*, for being the very oppoſite to a real analytical proceſs;—for adopting as " the fundamental propoſition, as a firſt principle, a theorem which in fact is nothing

" more than a ſagacious obſervation of an univerſal fact, diſcoverable indeed in every " mechanical phenomenon; but ſtill not a principle, but the mathematical and not the " phyſical reſult of all our inductions. This is not a fundamental theorem, fit for in- " ſtructing a novice in the ſcience, but for adepts alone. The caſe is the ſame in che- " miſtry.

" But this is not the greateſt fault in the arrangement which ſets out from the conſti- " tution of the atmoſphere. In order to get the proofs on which the validity of this firſt " principle muſt entirely reſt, we muſt fall to work with a number of complex, very " complex ſubſtances, of which we know nothing, and whoſe modes of action are " among the moſt myſterious things in chemiſtry; and the concluſions which we muſt " draw require a ſteadineſs and contention of thought which very few poſſeſs,—which a " beginner in philoſophical inveſtigation cannot poſſibly poſſeſs. It is by no means fair " to appeal to a Lavoiſier, a Cavendiſh, or a Berthollet, or other great chemiſt, for the " clearneſs of the evidence. They are not the proper judges. Lay it before a ſenſible " metallurgiſt, ignorant of chemiſtry. Aſk this man whether he ſees the incontrovert- " ible force of the proof. When I take the matter in this light, I affirm that, even to a " philoſopher, the proofs of the fundamental propoſitions which have been acquieſced " in by the authors of this arrangement, are very ſcanty, very ſlight, and very refined. " This is a fault in a ſyſtem publiſhed for the inſtruction of the ignorant; and, in the " preſent day, it is a very great fault. There is juſt now a rage for ſyſtem *,—for " complete ſyſtems. We have got ſuch a high conceit of our knowledge, that we can- " not be pleaſed with a ſyſtem which acknowledges any imperfection: It muſt not leave " one open link: It muſt not leave any thing unexplained. And I ſee it always happen " that if the application of a ſyſtem to the explanation of phenomena be very compre- " henſive, leaving no blanks, and if the explanation have ſome feaſibility, this catches " the fancy,—it dazzles the underſtanding. Nay, we think it impoſſible that a principle " that is falſe can tally with ſo many phenomena. This ſeeming coincidence is conſi- " dered as a proof of its validity; and we are no longer ſolicitous about the *direct* proofs " adduced in the beginning. I have often heard ſuch arguments for what I knew to " be great nonſenſe. This kind of authority accruing to a theory from its ſpecious and " extenſive application to phenomena, is always bad; and, with mere beginners in phi- " loſophy, it is doing them an irreparable hurt. It nouriſhes that itch for theory; and " it makes them unſolicitous about the firſt foundations of it;—thus it forms in their " minds the worſt of all philoſophical habits.

" I am reſolved to go on in a very different way. I ſubſcribe to almoſt all Mr. " Lavoiſier's doctrines; and I will teach them all. And I affirm that I ſhall teach " them with an impreſſion of their truth which his method can never make. My ſtu-

* Much of what follows is taken from an introductory lecture with which Dr. Black was accuſtomed to begin his courſe.

" dents ſhall get all theſe doctrines piece-meal;—every one of them by ſteps which " ſhall be quite eaſy and confident, becauſe they ſhall be acquainted with every ſub- " ſtance before I employ its phenomena as proofs. Each of Mr. Lavoiſier's doctrines " ſhall ariſe in courſe, as a ſmall and obvious addition to the properties of ſome ſub- " ſtance already known. Then I ſhall carry the ſtudent back, and ſhew him that the " influence of our new diſcovery extends alſo to thoſe ſubſtances which we had been " conſidering before. Thus, all the doctrines will be had eaſily, familiarly, and with " confidence in their truth.

" I even think that this method will be more pleaſant,—the novelties, or reformations, " being, by this method, diſtributed over the whole courſe. And it will have yet another " advantage: It will make the ſtudent acquainted with the chemiſtry of former years, " which is far from being unworthy of the attention of a philoſopher. Newton, Stahl, " Margraaf, Cramer, Scheele, Bergmann, were geniuſes not below the common level. " But the perſon who learns chemiſtry by Lavoiſier's ſcheme may remain ignorant of all " that was done by former chemiſts, and unable to read their excellent writings.

" I do not find that my old arrangement needs much change: Some I will make,— " chiefly in the order in which I treat the inflammable ſubſtances and the metals."

Such was very nearly the manner in which my excellent preceptor expreſſed his ſentiments and intentions on the occaſion that I mentioned above, and at ſeveral other times. I cannot but think that he had much reaſon for what he ſaid. The proofs which were acquieſced in at that time were very ſcanty indeed, and very refined, unleſs they were deduced from known properties of ſubſtances, which can be familiar only to an adept in chemiſtry; and the daſhing boldneſs with which the chemiſts applied them to the moſt complicated phenomena, and the authority with which our acquieſcence was demanded, were diſguſting; particularly to a man of Dr. Black's ſcrupulous caution, which ſcarcely ever allowed him to hazard a conjecture. The public has great reaſon to regret that this ſagacious philoſopher, and faithful follower of Newton's ſteps, did not live to put the finiſhing hand to the arrangement of facts on which chemical ſcience ſhould be reſted.

Dr. Black's feeble conſtitution, and low ſtate of health for ſeveral years before his deceaſe, put this out of his power. The cloſe thinking which ſuch a taſk required, not only fatigued him, but greatly deranged his health; and he was always obliged to give it over. He employed himſelf, however, from time to time, in making ſmall changes in the arrangement of the articles of his courſe. In the laſt converſation I had with him on chemical ſubjects, he told me that he was about to make a general reviſion of his treatment of the metallic ſubſtances, in order to put into a more regular and connected form the way of conſidering each metal in different ſtages of oxydation. It does not appear that he had proceeded far in this reviſion.

I regret alſo that no diſtinct notes are to be found reſpecting the difficulties which Dr. Black found in the antiphlogiſtic ſyſtem of chemiſtry. He remarked, in ſome of his

annual courſes of lectures, that Mr. Lavoiſier's ſyſtem did not explain the light which is ſo characteriſtic of combuſtion; and ſaid that ſome very judicious obſervations on this ſubject were to be found in Lubbock's inaugural diſſertation, *de Principio Sorbili*, which alſo profeſſes to give a theory of combuſtion and acidification.

[*Note* 27. *p.* 350.]

There are innumerable facts which ſhew that common air is contained in fluids, chemically, but ſlightly combined. When water is gradually heated in a glaſs veſſel, or is put under an air-pump receiver, and the preſſure of the atmoſphere on its ſurface is removed, innumerable bubbles are formed all over it, and riſe to the top. If, on the other hand, the water be put into a condenſing apparatus, and be agitated for a long while with air violently compreſſed, we ſhall obſerve that as ſoon as we open the cock and let out the condenſed air, air-bubbles will form in the water in the ſame manner, and riſe up and eſcape. If the water, and the air which come out in the firſt experiment, occupy the whole phial, the air being now collected in one maſs at the top, the water will reabſorb it all after ſome time.

This evident mixture of air and water is perfectly tranſparent; and it appears by Mr. Canton's experiments that it is no more compreſſible than water purged of all air. The air, therefore, is not in its elaſtic ſtate; it is chemically combined.

In thoſe experiments, the air bubbles form moſt copiouſly on the ſides of the veſſel, or on any body immerſed in the water. Part of this probably was adhering to thoſe ſolids. This, however, is not certain from this appearance alone, becauſe the ſuperior attraction of the water and the glaſs ſhould detach the air diſſolved in the water contiguous to the glaſs ſooner than in any other place. But it is in part owing to air adhering to the glaſs: For when we drive out the air from the mercury in the upper end of a barometer by boiling, and then bring the ſame mercury in contact with a part of the tube farther down, we ſhall obtain more air from it. This muſt have adhered to the tube. I have alſo found that I could obtain a much greater quantity of air by boiling, or the air-pump, from water into which I had thrown a quantity of powdered glaſs. It is very probable, therefore, that a very conſiderable portion of the air which eſcapes from a ſolution of ſalt in water proceeds from the ſalt, and had been adhering to its ſurface.

[*Note* 28. *p.* 353.]

The general appearance of a clouded atmoſphere is extremely agreeable to this theory. It generally conſiſts of extenſive ſtrata of clouds, of no great thickneſs, ſeparated by ſtrata of clear air, of much greater thickneſs. The under ſurfaces of the clouds are pretty

even. This is preciſely what ſhould happen. One great ſtratum of air, coming from one quarter of the heavens, glides over another, coming perhaps from another quarter. Theſe, having a relative motion, rub as it were on each other, and mix a little way into each other, producing a precipitation, that is, a fleecy cloud; but the interior parts of the ſtrata of air which do not mix, remain clear. This conſtitution agrees alſo finely with the electrical phenomena of the atmoſphere, giving an extenſive conducting coating to the ſtrata of air which are in oppoſite ſtates of electricity. A thunder clap is ſoon ſucceeded by another, in the ſame place, becauſe it only reſtores the electric equilibrium in a certain, perhaps ſmall, thickneſs in the clear ſtrata of air; and the oppoſite electricities ſtill remaining in the reſt of the ſtratum, ſoon enable the coatings to give another diſcharge. It is like the reſiduum ſhock from glaſs that is coated only in ſpots. When an irregular wind toſſes the ſtrata, and jumbles them together, then we ſee great roundiſh clouds of a vaſt thickneſs vertically. Theſe clouds are probably clear within, and only ſuperficially opaque, viz. in the touching boundaries of the maſſes of air ſo jumbled, &c.

[*Note* 29. *p.* 365.]

It is not very clear in what caſe we have a proof of a greater, and in what of a leſſer attraction in the cryſtallization of a ſalt. It is true, the ſalt is let fall by the water, and one attraction, or principle of union, has prevailed over another. But, ſince a ſaline cryſtal is a compound of ſalt and water, the formation of a cryſtal is rather the evidence of a prevailing attraction between the ſalt and water. Why it takes place at a particular proportion of the ingredients, ſeems owing to the ſame cauſe as the regular forms which the cryſtals aſſume. It was obſerved on a former occaſion, that the form of ſolidity required the attracting forces to be different in different directions, and perhaps in the different parts of the particle; ſo that two particles of a cryſtal may tend to unite in one way rather than in another. Suppoſe a particle of a ſaline cryſtal to conſiſt of one particle of ſalt, and one of water, and ſuppoſe that they unite becauſe the particles of water attract thoſe of ſalt more ſtrongly than the particles of ſalt attract one another; the conſequence of this muſt be, that two ſuch particles, floating in the water, cannot unite indifferently. They will approach each other ſo that the ſaline ingredient of the one will apply itſelf, in preference, to the watery ingredient of the other. For ſimilar reaſons, even the two ingredients of a particle will unite in a particular manner. In ſhort, their manner of coming together will reſemble the manner in which magnets floating on quickſilver will come together. Polarity ſeems to be as good a term as any for expreſſing this affectation of a particular poſition or attitude. We have no authority for ſaying that the figure of a cryſtal muſt reſemble that of the primitive atom, although it undoubtedly depends much on that figure, becauſe that muſt greatly influence the inequality of the corpuſcular attraction in different directions and different points of the particle. It is

plain, and we ſee it in magnets, that this inequality muſt increaſe as we come nearer to the particle. This ſeems to be the effect of abſtracting the water of the ſolution. When this is done to a certain degree, the polarity begins to operate, and then the attracting points of the particles bring them briſkly together. It is not, therefore, a ſuperior ſtrength, but a certain modification of the corpuſcular force which produces the cryſtallization.

[*Note* 30. *p.* 370.]

Mr. Lowitz of the Imperial Academy of Sciences in Peterſburgh, has made ſome conſiderable improvement in the cryſtallization of ſalts. He makes the ſolution very warm, and when it is almoſt ready to ſhoot into cryſtals, he puts into it a ſmall cryſtal. The cryſtallization inſtantly begins, and goes on as the brine cools, producing beautiful cryſtals. As the brine becomes weaker, it begins to act on the cryſtal put in, and diſſolves it. He puts in another, which renews the cryſtallization,—and repeats this till all that will ſeparate is cryſtallized. The cryſtallization is more beautiful the more ſlowly we allow the brine to cool. It ſhould therefore be ſet in a tub of hot water. He obtained fine cryſtals in this way, even from the moſt deliqueſcent ſalts,—and ſucceeded with ſome that never had been cryſtallized before.

Mr. Lowitz, in the practice of this method, made an obſervation that is extremely curious, and peculiarly ſerviceable in this method of ſeparating ſalts of different kinds. Reflecting on Mr. Beaumé's having obtained pure cryſtals from a muddy ſolution, he thought it not improbable that a ſalt, in cryſtallizing, rejected any thing which did not correſpond with its own manner of going together, and therefore that a cryſtal of ſaltpetre would not affect a ſolution of Glauber's ſalt. He found it ſo upon trial. He then concluded that in a mixture of different ſalts, a cryſtal of one of them would induce the cryſtallization only upon its own kind. He diſſolved two ounces of ſaltpetre, and three ounces of Glauber's ſalt, in five ounces of water almoſt boiling hot. He divided the brine into three phials; and into one he put a cryſtal of nitre, into another a cryſtal of Glauber's ſalt. His expectations were completely anſwered. Nitre alone cryſtallized in the firſt, and Glauber ſalt in the ſecond, and a mixture in the third. Taking out theſe cryſtals, he then put into the remaining ſolutions a bit of the ſalt which had not cryſtallized. This immediately induced the cryſtallization.

This obſervation is equally important to the artiſt and to the philoſopher. It greatly aids the practical chemiſt in this troubleſome proceſs; and it is moſt inſtructive to the philoſophical chemiſt. It is not explained by the ſimple notion of things of one kind going together, of nitre uniting with nitre, and borax with borax; for this is not the operation. It is that nitre induces water to unite in the way of cryſtallization only with nitre; and borax diſpoſes it to unite only with borax. I think it one of the ſtrongeſt confirmations of the operation of thoſe peculiar modifications or compoſitions of corpuſcular attraction, which I have called *polarity*.

[*Note* 31. *p.* 428.]

Dr. Robert Hooke had publiſhed his theory of combuſtion in his *Micrographia*, in 1664, (page 103), in which he expreſsly ſays that combuſtion is nothing but the ſolution of the combuſtible body in this nitrous ſpirit, which conſtituted the greateſt part of our air; and in his *Lampas*, publiſhed in 1676, he again gives his theory at length, and obſerves that it had been very well received eleven years before by the learned at Oxford, and adopted by them in their writings, evidently alluding to Mayhow's work. It does not appear, however, that Hooke had evinced, by direct experiments, the preſence of this air in nitre, but that he had inferred it from the fact that air was neceſſary for the burning of all inflammables, and that nitre enabled any inflammable to burn without air, and muſt therefore have ſupplied it in the very act of combuſtion. We muſt acknowledge the juſtneſs of his inference, and admit him as the real original author of this theory of combuſtion. Mayhow is not more explicit, and certainly had never made any chemical experiments with the air obtained from nitre, which he ſeems to call the *nitro-aëreal* ſpirit. Had he tried it as the ſupporter of flame, the phenomena are ſo remarkable, that they would have been minutely deſcribed. Even in his explanation of combuſtion by this ſpirit, he is extremely obſcure; and it is impoſſible to decide whether his explanation is upon chemical principles, or by motion and mechaniſm. Dr. Hooke, on the contrary, is moſt ſimple and perſpicuous in his theory, which is purely chemical; and when he explains the phenomena of flame, he does it with as much preciſion and philoſophical propriety as can be conceived. Indeed, there is no where to be met with ſo accurate a deſcription and explanation of the flame of oil or ſpirits, as is to be ſeen in Hooke's *Lampas*, page 5. &c. His numerous writings on many parts of natural philoſophy give him many occaſions of applying this theory, and it is therefore moſt ſtrange that it ſhould have been ſo entirely forgotten. As he promiſes to bring his theory into better form, and undoubtedly made many notes on the ſubject, I am perſuaded that ſome of them will yet be found in that cheſt of papers which was delivered by his friend Mr. Waller to the Royal Society.

[*Note* 32. *p.* 433.]

A groove eight lines wide, four deep, and 386 feet long, was filled with good powder. It employed thirty-three ſeconds in burning from one end to the other. It was filled again, and then covered with deal boards, ſimply laid on it. Ten feet of the covering weighed one pound. The train now burnt from end to end in ſeven and a half ſeconds; ſo much was it expedited by this ſlight preſſure, which obliged the flame to blow through among the grains. It made no ſenſible noiſe. I was within a quarter of a mile of a magazine of 350 barrels of gunpowder, in a large tent: It caught fire and blew up. The

exploſion did not laſt above a ſecond, and was prodigiouſly loud. The barrels were the only confinement in this caſe, yet ſufficient for making the exploſion almoſt inſtantaneous.

The compoſition juſt now deſcribed by Dr. Black does not fire till all is fluid,—therefore all in the ſame ſtate,—and therefore all fires at once. The firſt expanſion of this compoſition appears to be immenſely greater than that of gunpowder. Half an ounce fired on an ordinary parlour fire-ſhovel will generally beat the place where it lay into a hollow. Half an ounce of gunpowder fired on a card, or even a bit of ſtiff paper, will not tear it. Yet pulvis fulminans, when incloſed in the chamber of a muſket, and exploded with a ball lying above it, will impel it with very little force. This ſhews that the expanſive force of the elaſtic matter produced in the exploſion diminiſhes much more by an increaſe of bulk than in the caſe of gunpowder.

[*Note* 33. *p.* 467.]

In a bundle, marked "old notes, excerpts, &c." I find ſome experiments on this ſalt, which deſerve ſome notice. Dr. Black ſeems to aim at the beſt proceſs for preparing it for medical purpoſes by ſublimation. He was aiming at the ſame thing with the ſalt called *acetous ammoniac*, and is ſurpriſed that he can prepare the latter very eaſily, but that the nitrous ammoniac could not be condenſed, although its ingredients are not nearly ſo volatile as thoſe of the other.

In one experiment with a dry nitrous ammoniac, which he had prepared himſelf by mixing colourleſs nitrous acid with the pureſt volatile alkali, the vapours were incoercible *in part*, and what did condenſe was almoſt pure water, greatly exceeding in quantity what could be ſuppoſed neceſſary for the cryſtals of the ſalt. He was obliged to give a paſſage to the incondenſable vapours. He tried whether they were inflammable, by preſenting a bit of lighted paper to the hole in the luting. It did not take fire, but it made the paper burn with prodigious violence. He thought, from this circumſtance, that it was the nitrous acid: But, putting alkaline ley into the receiver, he did not find it condenſe more readily, nor produce a nitre. Putting lime water into the receiver, he found no precipitation, when he uſed the pure ſalt prepared by himſelf; but employing ſome gotten in the ſhop of Mr. Hill, (which I mention, becauſe it ſhews the date of the experiment to be previous to 1766) he had precipitate. He attributes this to impure alkali, containing inflammable matter. He ſays, that although the incoercible fumes filled the laboratory, the effect on his breathing and ſenſations was *very far from being unpleaſant*. He ſuſpects that the acid ſuffers ſome decompoſition, and again wonders at the quantity of water obtained. With ſome particular view, he had mixed with the ſalt thrice its weight of finely pounded glaſs. In three ſucceeding trials, the mixture detonated, and burſt the veſſels, although the heat was not much above that of boiling water.

Theſe experiments tally, in many particulars, with thoſe in the judicious analyſis of this

ſalt by Mr. Davy of the Royal Inſtitution. Dr. Black has had ſome experience of the wonderful effects of the *gaſeous oxyd of azote*, or *nitrous oxyd*, which have given ſo much amuſement of late, and from which ſuch mighty medical conſequences are expected by ſome phyſicians. This ſubject will come before us ſome time hence.

[*Note* 34. *p.* 494.]

Dr. Black highly approved of a ſyſtematic nomenclature, and thought the French one extremely ingenious, and that its many barbariſms and philoſophical incongruities ſhould be overlooked, as ſomething unavoidable, or that they ſhould be corrected. Accordingly he occupied himſelf a good deal on this ſubject; but his notes are ſo imperfect, and, I may ſay, *undecided*, that I could not make any uſe of them. He diſapproved however exceedingly of the entire ſubſtitution of this for all other denominations of chemical ſubſtances; and affirmed that proper names, where they can be had, ſhould on all occaſions be preferred. The employment of the ſcientific names only gives an appearance of knowledge without the reality of ſcience. It is merely an abbreviation of language. There is the ſame neceſſity of learning that the muriat of ſoda is common ſea ſalt as that ſea ſalt is the muriat of ſoda. Without the laſt, indeed, you are not a chemiſt; but without the firſt your chemiſtry is of no uſe. He was, therefore, for retaining all the old names that were, ſtrictly ſpeaking, proper names; ſuch are, kali, muria, ſoda, natrum, nitrum. He thought air as good a name as gas, and combuſtion as chemical a phenomenon as oxygenation.

A determination to be the founder of a ſyſtem and a ſect of philoſophers ſeems to have ſeduced Mr. Lavoiſier, and made him acquieſce in meaſures which may be called violent and unbecoming. As for the *imitatores*, *ſervum pecus*, they boggled at no incongruity with common language and ſentiment; and rather had a pride in it, as a mark of their authority over the opinions of other men. What can be more abſurd than to give the name oxygenation to the formation of taſteleſs water or charcoal, or of combuſtion to phenomena where neither heat nor light are obſerved? No knowledge whatever is acquired by the excluſive uſe of this nomenclature; and it has introduced into chemiſtry the ſame licentious dialectic that the Ariſtotelian metaphyſics introduced into the ſchools of philoſophy, and will produce the ſame bigotry and the ſame ignorance. It gives the appearance of reſearch to mere technical language; and many pages of modern ſyſtems of chemiſtry "are but the naming of their tools."——HUDIBRAS.

Not only no acquiſition is made of knowledge, but if the theory be erroneous in any circumſtance, (and what philoſopher will ſay that this is impoſſible?) the error muſt inſeparably adhere to every name, and every phraſe, and every opinion.

But all this is only the licence of literary ambition and vanity. The ſame principles, and the very ſame men, formed this nomenclature and the new kalendar and metrical

ſyſtem of the French. But theſe inconveniences are not eſſential to a good ſyſtematic nomenclature. Had all the proper names been retained, and had a becoming deference been paid to ordinary language and ſentiment, the nomenclature would have been much more intelligible,—would be free from paradox,—and Mr. Lavoiſier's well deſerved honours would have been fully ſecured to him. Newton ſtill ſtands at the head of philoſophers, although they ſtill ſpeak of the ſun's path round the heavens, and retain the old aſtronomical language. But Newton had no ſuch ambition; nor did the Royal Society furniſh ſuch a *legion of honour* as Mr. Lavoiſier found among his countrymen.

END OF VOLUME I.

Printed by Mundell & Son, Edinburgh.